IN THE UNITED STATES DISTRICT COURT
FOR THE WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION

Chelsey Nelson Photography LLC, and **Chelsey Nelson,**

Plaintiffs,

v.

Louisville/Jefferson County Metro Government; Louisville Metro Human Relations Commission–Enforcement; Louisville Metro Human Relations Commission–Advocacy; Verná Goatley, in her official capacity as Executive Director of the Louisville Metro Human Relations Commission–Enforcement; and **Marie Dever, Kevin Delahanty, Charles Lanier, Sr., Leslie Faust, William Sutter, Ibrahim Syed,** and **Leonard Thomas,** in their official capacities as members of the Louisville Metro Human Relations Commission–Enforcement,

Defendants.

Case No. 3:19-cv-00851-BJB-CHL

Plaintiffs' Motion to Exclude Testimony of Netta Barak-Corren

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Introduction

Louisville proposes to offer testimony from Professor Netta Barak-Corren to show that a ruling for Plaintiffs Chelsey Nelson and her photography studio will cause public accommodations in Louisville to discriminate more. But this testimony should be excluded because her conclusions are speculative, her methods are unreliable, and her findings are irrelevant.

Barak-Corren's testimony includes a report, two articles, and an Appendix (collectively "*Masterpiece* Study").[1] The *Masterpiece* Study tried to prove that *Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm'n (Masterpiece*), 138 S. Ct. 1719 (2018) caused creative professionals in Indiana, Iowa, North Carolina, and Texas to discriminate more against same-sex couples. But to get there, the study assumes that these professionals viewed positive media reports about *Masterpiece*, believed that *Masterpiece* granted a religious exemption, and then felt emboldened to begin discriminating. Then the study assumes that professionals in Louisville will respond the same way if this Court rules for Chelsey.

But problems abound. *Masterpiece* didn't grant a religious exemption, there's no evidence about anyone's exposure to *Masterpiece* or media about *Masterpiece*, Barak-Corren cannot measure pre-*Masterpiece* discrimination, and Indiana, Iowa, North Carolina, and Texas are not Louisville. On top of those fatal flaws, the *Masterpiece* Study depends on unreliable methodology, lacks any evidence about Louisville, and never gauges long-term attitude-changes that would make any conclusions reliable today.

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[1] The articles are A License to Discriminate? The Market Response to Masterpiece Cakeshop, 56(2) Harvard Civil Rights–Civil Liberties Law Review (forthcoming 2021) (HCRCL) and Religious Exemptions Increase Discrimination Towards Same–Sex Couples: Evidence from Masterpiece Cakeshop, Journal of Legal Studies (forthcoming 2021) (JLS). The report (NCB Report), HCRCL, JLS, and the Appendix are attached Exhibits A, B, C, and D respectively.

For these reasons and more, Barak-Corren's testimony is speculative, unreliable, and irrelevant. Her testimony and the study should be excluded.

Statement of Facts

The *Masterpiece* Study tried to evaluate the effects of religious exemptions on creative professionals' willingness to provide services for same-sex weddings. HCRCL 1, 24. Barak-Corren focused on the *Masterpiece* decision because she thought the Supreme Court "would grant an exemption" and anticipated "extensive" media "coverage and discussion." *Id.* at 24.

But *Masterpiece* found "government hostility and did not reach the question of whether Phillips had a right to an exemption …." *Id.* at 18 n.56. So Barak-Corren had to pivot. She tweaked her planned study to evaluate *how the media communicated about Masterpiece* to the public. HCRCL 25–27; JLS 8–9; Transcript of Deposition of Netta Barak-Corren (Tr.) 189:4–16. Barak-Corren cites "conservative," "mainstream," and "progressive" media. HCRCL 25–27.

For her study, Barak-Corren evaluated photographers, bakers, and florists in Iowa, Indiana, North Carolina, and Texas. *Id.* at 29; JLS 14–16. She selected these states because they had "comparable … overall characteristics" but "differed in legal regime." *Id.* at 29. "Legal regime" describes jurisdictions with or without (+/–) state religious freedom restoration act (RFRA) laws and jurisdictions with or without (+/–) state or local antidiscrimination (AD) laws. JLS 12–14. Selecting different legal regimes was "necessary" to uncover "real-world variation." *See* HCRCL 32.

About a month before *Masterpiece*, Barak-Corren emailed 906 creative professionals from fictitious same-sex couples (Wave 1) and then from fictitious opposite-sex couples (Wave 2) asking about wedding services. HCRCL 39; JLS 3, 17–18; Appendix (App.) 1–4. Many more professionals responded to the same-sex couples (70.8% response rate) compared to the opposite-sex couples (58.7% response

rate). HCRCL 36; JLS 21. Because of this "attrition" between Waves 1 and 2, Barak-Corren admits that she cannot know the "the extent of discrimination towards same-sex couples … before *Masterpiece*." Tr. 147:12–21, 156:7–23.

After *Masterpiece*, Barak-Corren changed her email content and emailing strategy. HCRCL 34; App. 1–10. Rather than send the emails in block waves (a wave from same-sex couples and another wave from opposite-sex couples), she randomized inquiries in each wave so that half of the professionals in wave 3 received inquiries for a same-sex wedding and the other half received inquiries for an opposite-sex wedding. In the final wave (Wave 4), each professional then "received an email from the opposite–orientation couple" than they received in Wave 3. HCRCL 34. Barak-Corren also called dozens of creative professionals in between Waves 3 and 4. App. 11–14.

After collecting this data, Barak-Corren tried to study *Masterpiece*'s effect by evaluating whether "businesses that agreed to serve same-sex couples before the decision" agreed to provide the service after the decision. HCRCL 38; JLS 19. She did this by coding positive and negative responses. The upshot is that she considered non-responses as negative responses. *See* JLS 19 n.25; App. 29.

Barak-Corren sent over 3,600 emails. App. 19 (noting "N" as 906 times four waves). No professional explicitly declined an email request from a same-sex couple because they "don't do same-sex weddings." Tr. 199:4–10. She also spoke with seventy-three professionals. App. 14. Only one professional declined a phone inquiry from a same-sex couple because he or she "only d[id] traditional weddings." *Id.*

Non-responses were "[t]he most common form of declining service." HCRCL 38. Because the number of explicitly negative responses or negative responses with referrals were so "small," Barak-Corren didn't "base any … statistical inference only on those numbers." Tr. 128:3–5 So non-responses were the "driver" in measuring pre-and-post *Masterpiece* responses. Tr. 131:6–14.

From that, Barak-Corren found that *Masterpiece* had a "negative effect … on the willingness to provide services to same-sex couples." HCRCL 39. This effect occurred in all legal regime types, except in areas with a state RFRA and a local AD law (+RFRA/+AD). *Id.* at 41–42.

Taking all this together, Barak-Corren believes that a finding for Chelsey will "significantly increase the likelihood" that wedding professionals in Louisville will decline to provide services for same-sex weddings. NCB Report ¶ 12. George Yancey, Ph.D. wrote a rebuttal report (GY Report, attached as Exhibit E). For the reasons explained below, Barak-Corren's testimony should be excluded.

Argument

Courts act in "a gatekeeping role" over expert testimony. *Daubert v. Merrell Dow Pharms., Inc.*, 509 U.S. 579, 597 (1993); *Kumho Tire Co. v. Carmichael*, 526 U.S. 137, 147 (1999). In this role, courts evaluate whether the testimony is reliable, is relevant, and comes from a qualified expert. *In re Scrap Metal Antitrust Litig.* (*Scrap Metal*), 527 F.3d 517, 528 (6th Cir. 2008) (citing Fed. R. Evid. 702). This evaluation can be made with or without a hearing. A hearing is unnecessary here because this brief and the record form "an adequate basis" on which to decide this motion. *Nelson v. Tennessee Gas Pipeline Co.*, 243 F.3d 244, 249 (6th Cir. 2001).

Louisville must establish that Barak-Corren's testimony is admissible "by a preponderance of proof." *Id.* at 251. Louisville cannot do so. Barak-Corren's testimony is (I) unreliable because it rests on speculation and flawed methodology and (II) irrelevant to Chelsey's constitutional and statutory claims.

I. Barak-Corren's testimony should be excluded as unreliable.

Barak-Corren's testimony should be excluded because it is unreliable. Federal Rule of Evidence 702 gives three reliability requirements. *Scrap Metal*, 527 F.3d at 529. *Daubert* provides more guidance. *Kumho*, 526 U.S. at 150. Expert

testimony must be (A) based on "reliable principles and methods"; (B) "based on sufficient facts or data"; and (C) applied in a reliable way. Fed. R. Evid. 702(b)–(d); *Scrap Metal*, 527 F.3d at 529. Barak-Corren's testimony fails these requirements.

A. Barak-Corren's testimony rests on unreliable principles and methods because she relies on speculation and flawed methodology.

Barak-Corren's testimony is not reliable because it depends on (1) unverified assumptions about professionals' knowledge of *Masterpiece*; (2) inaccurate characterizations of legal regimes; (3) unknown rates of pre-*Masterpiece* same-sex discrimination; and (4) problematic auditing techniques.

1. Barak-Corren speculates about media consumption without any defined methodology.

The *Masterpiece* Study was designed to measure "the effects of religious exemptions on discrimination towards same-sex couples." HCRCL 24. But to measure this, Barak-Corren relies on *what the media said* about the *Masterpiece* decision and *what people understood* the opinion to say based on those media reports. *Id.* at 24–27, 47–48 n.150 (relying on media for "expressive theory of law"); Tr. 189:4–14. This approach contains many errors: Barak-Corren never measures creative professionals' exposure to media or to the *Masterpiece* decision itself, never characterizes the media creative professionals saw (if any), and never explains how she selected the media to exemplify reporting on the opinion.

For starters, Barak-Corren never investigated "whether the vendors [she] contacted … had any knowledge of, appreciation for, or understanding of the *Masterpiece* decision after it was rendered." Tr. 93:23–94:11. Even so, Barak-Corren concludes that *Masterpiece* caused creative professionals to become less willing to provide services for same-sex weddings. *See, e.g.*, NCB Report ¶ 14; HCRCL 2; *id.* at 47 (arguing *Masterpiece* changed professionals' "perceptions of the social norms

regarding service refusal"); JLS 4. But *Masterpiece* could only cause this outcome if creative professionals knew about the opinion. That's common sense.

But Barak-Corren cannot measure this causation because she cannot verify anyone's exposure to *Masterpiece*. *See* GY Report ¶¶ 30–32. Her study cannot decipher what professionals understood about *Masterpiece* or whether media they followed described this decision as granting a religious exemption. *Id.* If anything, most media likely reported on the decision narrowly or critically. *Id.* at ¶ 31; HCRCL 25–27 (citing "mainstream" and "progressive" media). Because Barak-Corren cannot know anyone's exposure to *Masterpiece*, she can only speculate that professionals declined to provide services for same-sex weddings *because of* the decision. This speculation is unreliable. *Cf. Nelson*, 243 F.3d at 252–53 (concluding expert's "reasoning and methodology … [was] not scientifically valid" because he determined causation by speculating about plaintiff's exposure to toxins).

Barak-Corren relies on four studies to fill her knowledge gap.[2] Two of these studies prove Barak-Corren's causation problem. The first (Linos and Twist) said subjects must know about Supreme Court decisions to be influenced by them.[3] And

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[2] These studies are (1) Katerina Linos & Kimberly Twist, *The Supreme Court, the Media, and Public Opinion: Comparing Experimental and Observational Methods* (Linos & Twist), 45 J. Legal Stud. 223 (2016) (attached as Exhibit G); (2) Margaret E. Tankard & Elizabeth Levy Paluck, *The Effect of a Supreme Court Decision Regarding Gay Marriage on Social Norms and Personal Attitudes* (Tankard & Paluck), 28 Psych. Sci. 1334 (2017) (attached as Exhibit H); (3) Emily Kazyak & Mathew Stange, *Backlash or a Positive Response?: Public Opinion of LGB Issues After Obergefell v. Hodges* (Kazyak & Stange), 65 J. Homosexuality 2028 (2018) (attached as Exhibit I); and (4) Eugene K. Ofosu et al., *Same-Sex Marriage Legalization Associated With Reduced Implicit And Explicit Antigay Bias* (Ofosu), 116 Proc. Nat'l Acad. Sci. U.S. 8846 (2019) (attached as Exhibit J). *See* HCRCL 27-28 nn. 96–99 (citing these studies); Tr. 95:1–96:22 (same).

[3] 45 J. Legal Stud. at 231 (connecting "individuals' opinion shifts to" media "which they were (or were not) exposed to"); *id.* at 239 ("It is, therefore, important to not only measure aggregate opinion change but distinguish people who heard and understood the decisions from those who did not.").

the second (Kazyak's and Stange's study on *Oberfell*) asked "whether respondents heard or read about the [ruling]" and confirmed "the vast majority of respondents were aware of the ruling." 65 J. Homosexuality at 17.

The other two studies are irrelevant. Tankard and Paluck measured "perceived (present and future) social norms in support of gay marriage," not "personal attitudes toward gay marriage or in ratings of gay people," which would affect behavior. 28 Psych. Sci. at 1339. And Ofosu measured how legislation legalizing same-sex marriage changed attitudes towards same-sex marriage over a twelve-year period, not how judicial opinions changed personal decision-making in two weeks. 116 Proc. Nat'l Acad. Sci. U.S. at 4-5. Ultimately, Barak-Corren offers no evidence about creative professional's exposure to the *Masterpiece* decision.

But even if Barak-Corren measured media exposure (she did not), she missed another step by not isolating what media professionals actually saw. Barak-Corren identifies "mainstream outlets," "conservative leaders and religious liberty advocates," and "progressive commentators" as media sources. HCRCL 25–27. "[M]ainstream" and "progressive" outlets classified Masterpiece narrowly or critically. HCRCL 25–27. "[C]onservative" media "hailed the decision as a victory" and "express[ed] significantly less reservations about its scope." HCRCL 25.

These broad strokes gloss over important nuances that would dictate what professionals could have learned about the case. For example, "mainstream," "conservative," and "progressive" media presented *Masterpiece* differently. *Id.* 25–27. News coverage like this—with both "supportive and critical information"—is known as "two–sided coverage." Linos & Twist, 45 J. Legal Stud. at 225–26. And two–sided coverage "reduce[s] the impact of the Court decision on opinion change." *Id.* So creative professionals that saw mainstream *and* conservative coverage (or nuanced coverage) about *Masterpiece* would be less likely to change their opinion about providing services for same-sex weddings. *See id.*

Similarly, even professionals that only viewed "conservative" media wouldn't come to the same conclusions about the decision. The truth is that most of the "conservative" articles either reported the decision as a religious hostility case,[4] never mentioned a religious exemption,[5] or included information criticizing the opinion.[6] HCRCL 25–26 nn.89–92 (citing most articles in footnotes 4–6 below).

Finally, even if Barak-Corren measured media exposure (she does not) or isolated what media reports professionals were exposed to (she does not), Barak-Corren's reliance on the media to communicate about *Masterpiece* would still be unreliable because her study is not replicable. *See, e.g.*, *Zenith Electronics Corp. v. WH-TV Broadcasting Corp.*, 395 F.3d 416, 419 (7th Cir. 2005) (reliability requires "[s]omeone else using the same data and methods … to replicate the result").

For example, Barak-Corren never explains why she selected certain media as exemplifying how the media in general communicated about *Masterpiece*. *Cf.* JLS 11–20 (describing methods but omitting media selection). Why did she pick the New York Times over the Washington Post? *Id.* at 25. Or how did she determine that press releases from the U.S. Conference of Catholic Bishops, Liberty Counsel, or the Family Research Council qualified as media reports at all? HCRCL 25–26 (citing

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[4] *Victory for Colorado Cake Case*, Liberty Couns. (June 4, 2018), https://perma.cc/9M8L-QZ23 "[T]he Court focused on the explicit hostility ….").

[5] *Colorado Baker Reacts to 'Big Win' in Same-Sex Wedding Cake Case*, Fox News Insider (June 5, 2018), https://perma.cc/3Z2C-PDRP; Todd Starnes, *A win for Masterpiece Cakeshop but it ain't over yet*, Fox News (June 4, 2018), https://perma.cc/8STY-5Q5Z (noting "gay rights do not necessarily trump everyone else's rights"); *Religious freedom groups praise Supreme Court's Masterpiece ruling*, Cath. News Agency (June 4, 2018), https://perma.cc/NV9W-38UR ("'The Court's holding is narrow.' (quoting Brian Miller)).

[6] Bill Mears & Judson Berger, *Supreme Court sides with Colorado baker who refused to make wedding cake for same-sex couple*, Fox News Live (June 4, 2018), https://perma.cc/6YHF-XMS9 (calling ruling "narrow" and quoting David Mullins and dissenting justices).

sources as "conservative" media). We don't know. Barak-Corren doesn't explain why or how she selected the articles she relies on as exemplifying news reports about *Masterpiece*. This lack of any explained method for selecting media makes Barak-Corren's testimony unreliable because it cannot be repeated.

Likewise, Barak-Corren never explains why she categorized some media as "mainstream outlets," "conservative leaders and religious liberty advocates," and "progressive commentators" as media sources. HCRCL 25–27. *Cf.* JLS 11–20.

This lack of explanation contradicts the generally accepted practice of studying the media, the Supreme Court, and public opinion. *See Daubert*, 509 U.S. at 593 (general acceptance relevant to reliability). For example, Linos & Twist evaluated the Supreme Court's ability to shape public opinion. 45 J. Legal Stud. at 223; HCRCL 25 n.86 (citing this study). In their study, they followed the "six major television networks" coverage of several Supreme Court decisions because "television remains the main source of news for most Americans." Linos & Twist, 45 J. Legal Stud. at 223, 229. They also coded "evening news transcripts, sentence by sentence, classifying each sentence, or portion thereof in two ways" to determine whether the coverage was positive or negative. *Id.* at 233 n.5.

In contrast, Barak-Corren relies on "conservative" online articles from one major outlet (Fox News), one news website (Daily Signal), one religious news company (Catholic News Agency) and a smattering of unrelated press releases. HCRCL 25–26 nn.89–92.[7] Barak-Corren never codes the content of these articles—she just selectively quotes from them. *Id.*; JLS 9 n.10 ("survey[ing] key quotes").

In the end, Barak-Corren's causation analysis requires speculation because she has no evidence about what creative professionals knew or saw about the

[7] Only the "conservative" outlets are potentially relevant to professionals' exposure to *Masterpiece* as a religious liberty case. That's because "mainstream" and "progressive" outlets classified *Masterpiece* narrowly or critically. HCRCL 25–27.

opinion. Her speculation about causation is unreliable. *See Tamraz v. Lincoln Elec. Co.*, 620 F.3d 665, 669–72 (6th Cir. 2010) (causation testimony with speculation was unreliable); *Rodrigues v. Baxter Healthcare Corp.*, 567 F. App'x 359, 361 (6th Cir. 2014) (testimony unreliable without "causal link"); *Rose v. Truck Centers, Inc.*, 388 F. App'x 528, 535 (6th Cir. 2010) (testimony "must have an established factual basis" and not reliable when "premised on mere suppositions").

2. Barak-Corren's legal regimes undermine her conclusions.

Barak-Corren's findings about +RFRA/+AD jurisdictions also completely undermine her conclusions about creative professionals in Louisville.

Barak-Corren found that *Masterpiece* caused no change in professionals' willingness to provide services for same-sex weddings in +RFRA/+AD jurisdictions in Texas (and Indiana). HCRCL 41–42, 54–56; JLS 31–32. Professionals in these jurisdictions "were not merely more consistent in their behavior; *they were also the least discriminatory of same-sex couples post*-Masterpiece Cakeshop." HCRCL 54 (emphasis added). Barak-Corren suggests "the tension built into these hybrid regimes led businesses to reflect and contemplate their positions" before *Masterpiece*. *Id.* So "[h]aving already formed a position, businesses in hybrid regimes were possibly more resistant to the influence of" *Masterpiece*. *Id.*

But Louisville is a +RFRA/+ AD jurisdiction. *Compare* § K.R.S. 446.350 *with* Metro Ord. § 92.05. So Barak-Corren's study indicates that a decision like *Masterpiece* would not affect the willingness of Louisville professionals to provide services for same-sex weddings.

To be sure, Barak-Corren denies this conclusion, saying that Kentucky and Texas's RFRAs differ because Texas's does not provide a civil defense to local antidiscrimination laws, while Kentucky's does. NCB Report ¶¶ 15–16. But that's wrong. True, Texas's RFRA cannot be used as a defense "to a civil action … under a

federal or state civil rights law." Tex. Civ. Prac. & Rem. Code § 110.011(a). But this exemption doesn't apply to *local* civil rights laws. So Texas's RFRA provides a defense to local anti-discriminations laws (just like Kentucky's RFRA does). *See* Tex. Civ. Prac. & Rem. Code § 110.001(a)(2) (defining "government agency" to include "a municipality," including any "commission."). In fact, Texas federal and state courts apply Texas's RFRA against local laws. *See Merced v. Kasson*, 577 F.3d 578, 588–95 (5th Cir. 2009) (applying law to animal slaughter statute); *Barr v. City of Sinton*, 295 S.W.3d 287, 297–308 (Tex. 2009) (applying law to zoning ordinance). And when a group of churches sued Austin, Texas over its local antidiscrimination law for violating Texas's RFRA, Austin didn't even raise § 110.011(a) as a defense. *See* Defs.' Mot. to Dismiss Pls.' Compl., *U.S. Pastor Council v. City of Austin*, Case No. 18-cv-849-RP (W.D. Tex. Jan. 8, 2019), ECF No. 6 (attached as Exhibit O). If that provision made the RFRA inapplicable, Austin would have argued so.

Barak-Corren even admitted that her conclusions would be different if Chelsey lived in Austin (a +RFRA/+AD jurisdiction). Tr. 172:15–172:2; JLS 14 n.13 (classifying Austin). But Louisville and Austin have more in common than the "keep it weird" slogan. Tr. 173:3–14. The legal regimes are the same too. So Barak-Corren's admission is decisive. Her testimony proves that *Masterpiece* or a similar ruling would not affect creative professionals in Louisville.[8]

[8] Barak-Corren misclassified other jurisdictions too. For example, she labels Iowa as -RFRA/+AD. HCRCL 42. But Iowa's law requires public accommodations to have "a spatial dimension" or physical place. *See U.S. Jaycees v. Iowa C.R. Comm'n*, 427 N.W.2d 450, 454 (Iowa 1988). So creative professionals in Iowa who run online businesses aren't subject to Iowa's public accommodations law; they effectively operate in a -RFRA/-AD regime. Likewise, Barak-Corren labels Indiana as a +RFRA and +/- AD depending on the local jurisdiction. HCRCL 30. But Indiana's RFRA isn't a defense to a civil rights complaint. *See* HCRCL 19 n.64. So professionals in Indiana effectively operate in a -RFRA and + or – AD regime. Barak-Corren never accounts for these nuances.

3. Barak-Corren cannot measure pre-*Masterpiece* discrimination.

Barak-Corren's testimony should also be excluded because she concedes she cannot measure pre-*Masterpiece* discrimination.

Barak-Corren's study got off on the wrong foot. In Wave 1, 70.8% of professionals responded to same-sex inquiries. HCRCL 36. But in Wave 2, 58.7% of professionals responded to opposite-sex inquiries. *Id.* So before *Masterpiece*, more professionals responded to same-sex inquiries (Wave 1) than opposite-sex inquiries (Wave 2). This decline (the attrition rate) skews the entire study.

As Barak-Corren admits, the different response rates in Waves 1 and 2 "hindered the [study's] ability to detect discrimination in the pre-*Masterpiece Cakeshop* period." HCRCL 36-37. *See also* JLS 21, 23 n.29; Tr. 147:12–21. That alone shows the *Masterpiece* Study is unreliable.[9] Despite this admission, Barak-Corren tries to compare discrimination pre-and-post *Masterpiece* anyway. For example, Barak-Corren concludes that, after *Masterpiece*, "inquiries from a same-sex couple had a 66.3% chance of receiving a positive response" while "inquiries from an opposite-sex couple have a 75.5% chance of being answered positively." HCRCL 38. She attributes this change to *Masterpiece*. *Id.* But these statistics are meaningless without knowing the comparable rates of positive responses for same-sex and opposite-sex couples *before Masterpiece*. In other words, if positive response rates were the same (or worse) before *Masterpiece* (e.g., 66.3% for same-sex couples and 75.5% for opposite-sex couples) then *Masterpiece* had no effect (or a positive

[9] *See* Tankard & Paluck, at 1335 ("Understanding causal effects of [a Supreme Court] decision is difficult without the ability to randomize exposure … or without prospective time-series data testing how individuals change prior to and following" such a decision."); *id.* at 1338 (following "the same individuals prior to and following the June 2015 U.S. Supreme Court ruling on same-sex marriage"); Kazyak & Stange, 21 Homosexuality at *15-16 (containing pre-and-post *Obergefell* data to measure attitudes on same-sex marriage).

effect) on the different response rates. But Barak-Corren cannot know this information because of the attrition between Waves 1 and 2. There simply is no statistical baseline that can show that *Masterpiece* caused a decline in response rates from Waves 1 and 2 (pre-*Masterpiece*) to Waves 3 and 4 (post-*Masterpiece*).

Barak-Corren backpedals from this error by measuring businesses that "agreed to serve same-sex couples before the decision." JLS 23. But this workaround creates a "regression fallacy." James E. Ciecka, *The First Use of the Term Regression in Statistics* (Ciecka), 17 J. Legal Econ. 31, 38 (2010). Regression to the mean "describes a tendency of extreme measurements to move closer to the mean when they are repeated." Christy Chuang-Stein, *The Regression Fallacy* (Chuang-Stein), 27 Drug Information J. 1213, 1213 (1993) (attached as Exhibit P). So "in a test-retest situation, the bottom group on the first test will on average show some improvement on the second test while the top group will on average fall back." *Id.*; Colleen Kelly & Trevor Price, *Correcting for Regression to the Mean in Behavior and Ecology* (Kelly & Price), 166(6) Am. Nat. 700, *1 (2005) (attached as Exhibit Q) (explaining test-retest similarly). "Regression to the mean" is "present whenever individuals … are measured two different times." Kelly & Price at *2.

Here's how the fallacy applies. Barak-Corren relied on the "576 businesses that agreed to serve same-sex couples before the decision" in Wave 1 to measure post-*Masterpiece* discrimination. HCRCL 38. In other words, Barak-Corren pins the pre-and-post *Masterpiece* comparator on professionals who responded positively 100% of the time to same-sex inquiries in Wave 1. But these responses were not representative; by definition they were atypical in their positive responsiveness to same-sex inquiries. So the post-*Masterpiece* same-sex response rate from the professionals who responded positively to same-sex inquiries in Wave 1 likely underwent "regression to the mean" and moved closer to those professionals' average same-sex responsiveness. Stated differently, by starting with 100% positive

responses to same-sex inquiries pre-*Masterpiece*, professionals' responsiveness to same-sex inquiries had nowhere to go but down. So the post-*Masterpiece* move to non-responsiveness to same-sex inquiries was contaminated by the expected statistical corrections in that direction, i.e. regression to the mean.[10]

This contamination dooms the claim that the non-responsiveness was due to any intervening event. Kelly & Price, 166(6) Am. Nat. at *1 (noting fallacy "arises when a researcher attributes a decrease to an intervention"); Ciecka, 17 J. Legal Econ. at 38 (fallacy "is compounded when a cause is ascribed to something that is simply due to regression to the mean."). Without accounting for this regression to the mean, Barak-Corren's conclusions are "greatly exaggerated," Chuang-Stein, 27 Drug Information J. at 1213, and likely wrong.

The attrition conundrum also requires Barak-Corren to inconsistently code non-responses. Barak-Corren attributes non-responses in Waves 2 to "attrition." HCRCL 37; GY Report ¶ 29. She does this to avoid the conclusion that opposite-sex couples were discriminated against more than same-sex couples before *Masterpiece*. HCRCL 37 n.131 (admitting this point). But in Waves 3 and 4, Barak-Corren codes non-responses as negative responses. *See* App. 29. This raises the question: Why are non-responses before *Masterpiece* attrition and non-responses after *Masterpiece* discrimination? Barak-Corren never answers.

4. Barak-Corren codes non-responses as negative responses, producing unreliable results.

Barak-Corren's decision to code non-responses as negative responses causes trouble too. Barak-Corren admits that she relies on non-responses as the pre-and-

[10] For example, perhaps an event occurred in early May that increased the likelihood of a response. Or professionals may have been uniquely eager to respond to requests in May. The point is that whatever accounted for the high same-sex response rate in Wave 1 naturally dissipated over time under a regression theory.

post-*Masterpiece* comparator because there were too few explicitly negative responses or negative responses. Tr. 128:2–25. But a creative professional may not respond to an inquiry for many reasons.

Barak-Corren lists some of them in her phone survey of seventy-three professionals. App. 13–14. Of those professionals, four said they never received the email, two thought the email was a scam, and other professionals "provided various reasons for not responding" including "having intended to respond or being unable to provide the service." *Id.* at 14. Of course, some professionals "did not explain 3rd Wave non-response." *Id.* But the point is that many of the contacted professionals had legitimate reasons for not responding to the emails.

For example, creative professionals could have not responded because Barak-Corren used multiple emails to contact them multiple times. This causes fatigue and raises suspicion. GY Report ¶¶ 11–12. And it required Barrak-Corren to change the content of the emails. *Id.* ¶ 13. Barak-Corren ignores these changes. *Cf. Davis v. Landscape Forms, Inc.*, 640 F. App'x 445, 455 (6th Cir. 2016) (A "statistical report's failure to account for explanatory variables may make admissibility an uphill battle."). But these changes effected professionals' responses in several ways.

First, emails to professionals in Waves 1 and 2 asked about their availability in a general month while emails in Waves 3 and 4 asked for a specific date. App. 1–10. The fact that post-*Masterpiece* inquiries asked for exact dates likely depressed response rates compared to the pre-*Masterpiece* inquiries asking for a monthlong range. *See* GY Report ¶ 13. After all, if a professional was available for the requested month in Waves 1 and 2, she may have replied positively; if she wasn't available for the exact date in Waves 3 and 4, she would've responded negatively.

Second, emails in Waves 3 and 4 asked professionals for services on Friday *and* Saturday. Appendix 4–10. Barak-Corren never claims requests for two-day services are normal in the wedding industry.

Third, the same-sex couples' emails in Waves 3 and 4 asked for in–person meetings, while same-sex couples' emails in Wave 1 didn't. *Compare id.* at 1–4 *with id.* at 4–5, 7–9. Professionals may have been uncomfortable with an initial in-person meeting. These three changes "introduce[] the potential problem that professionals react differently to the contrasting ways the emails are worded" and "make it difficult to have any confidence in the report's findings." GY Report ¶ 13–14.

Next, the change in email content halfway through the study also forced Barak-Corren to change her response coding. Barak-Corren first coded referrals and suggestions of alternative dates as a positive response in Waves 1 and 2. App. 29 n.16. But she coded those same behaviors as negative responses in Waves 3 and 4. *Id.* The altered email content prompted the need to flip the coding of these behaviors. *Id.* (suggesting alternative dates in Waves 3 and 4 was "a negative response because it is common knowledge that a wedding date will not be changed for any single provider …"). But this change created a bias towards finding increased negative responses after *Masterpiece* because the same behavior is measured differently. And this bias has a disproportionate effect on inquiries for same-sex weddings because of the comparatively high response rate in Wave 1.

Finally, non-responses could be because of the timing of the email waves. Barak-Corren sent emails in Waves 1 and 2 in May and emails in Waves 3 and 4 in June. HCRCL 29. But many more Americans vacation in June than May.[11] And June is busier for wedding professionals than May; so professionals' comparatively busy calendars could explain their non-response.[12]

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[11] Keeneth Kiesnoski, *Here's where Americans are planning to go for that summer vacation*, CNBC (Apr. 21, 2021), https://cnb.cx/3AX1YHr (explaining 67% of Americans travel from June to August compared to 17% from March to May).

[12] The Knot, *What to Know About Wedding Season and the Off-Season* (Nov. 18, 2020), https://bit.ly/3ms0rFp (June is the third most popular wedding month).

Barak-Corren never teases out any of these legitimate reasons for non-responses. *See Pluck v. BP Oil Pipeline Co.*, 640 F.3d 671, 680 (6th Cir. 2011) (testimony excluded where expert failed to rule out alternative causes). She just lumps them into her general conclusion that "previously 'gay friendly' businesses … responded less favorably to same-sex couples than opposite-sex couples … after the decision was rendered." HCRCL 6–7, 38; JLS 24.

Instead, Barak-Corren counters that even though "non-responses may have had other causes … we would expect such errors to distribute randomly." HCRCL 38. Responses were not randomized, Barak-Corren says, because opposite-sex couples had a higher chance of receiving a response than same-sex couples. *Id.* Here the regression fallacy surfaces again. *See* § I.A.3. This comparison is reliable only if Barak-Corren knew pre-*Masterpiece* discrimination rates. She does not.

These issues contaminate Barak-Corren's testimony and make it unreliable.

B. Barak-Corren's testimony is not based on sufficient facts or data because she gives no evidence about how her study would apply in Louisville.

Next, Barak-Corren's testimony that granting a religious exemption for Chelsey in this case will increase discrimination in Louisville is unreliable without "facts or data" about Louisville's creative professionals. Fed. R. Evid. 702(b). Barak-Corren has none.

Barak-Corren stresses specific evidence. She claims efforts to balance religious liberty and same-sex marriage "should rely on … empirical evidence," counsels parties to "present *directly relevant data,*" and urges those reading her study to rest arguments "on relevant empirical evidence." HCRCL 55, 59, 63.

But Barak-Corren ignores this advice here. For example, Barak-Corren never measures the public's exposure to district court decisions or claims those opinions

receive the same media attention as Supreme Court decisions. *See supra* § I.A.1. That alone exiles the *Masterpiece* Study in this case before the district court.

She hypothesizes about how the *Masterpiece* Study might apply in Louisville based on outcomes in Iowa, Indiana, North Carolina, and Texas with no evidence from Louisville. *See* NCB Report ¶¶ 12–23. *See, e.g.*, *Newell Rubbermaid, Inc. v. Raymond Corp.*, 676 F.3d 521, 528–29 (6th Cir. 2012) (excluding testimony due to improper extrapolation of data from non-comparable forklifts); *Powell v. Tosh*, 942 F. Supp. 2d 678, 692 (W.D. Ky. 2013) (excluding expert evidence based on "extrapolation of the data" from neighboring farm). With no evidence from Louisville, Barak-Corren cannot determine how the *Masterpiece* Study applies to Louisville nor can she reliably compare Louisville to the studied states.[13] GY Report ¶ 20. This is especially true because of how few variables the *Masterpiece* Study measures and the "nested dataset" of the study. *Id.* ¶¶ 16-20.

Barak-Corren didn't audit professionals in Louisville. And Barak-Corren didn't research the attitudes of those in Louisville towards "homosexuals" or same-sex marriage or the percentage of "Conservatives" or "Evangelicals" in Louisville. NCB Report ¶ 22. She did those comparisons for Kentucky. But Louisville is not Kentucky. It is an "'apples and oranges' comparison." *Shatkin v. McDonnell Douglas Corp.*, 727 F.2d 202, 208 (2d Cir. 1984).

Louisville residents oppose same-sex marriage less often than Kentuckians. *Compare* PRRI, *The American Values Atlas*, https://bit.ly/3xSvpbL (last visited Aug. 30, 2021) (38% oppose in Louisville) *with* NCB Report ¶ 22 (52% oppose in Kentucky). And while more than half of Kentucky residents consider themselves to

[13] *See, e.g.*, *Anderson v. Westinghouse Savannah River Co.*, 406 F.3d 248, 262 (4th Cir. 2005) (excluding study in discrimination case that "failed to compare similarly situated workers"); *Raskin v. Wyatt Co.*, 125 F.3d 55, 67 (2d Cir. 1997) (excluding expert report based on "an unrepresentative sample for his comparison").

be conservatives, less than a third of Louisville residents are registered Republicans. *Compare* Jefferson County Clerk's Office, *KY Voter Registration Statistics*, https://bit.ly/3AX6Nk1 (last visited Aug. 30, 2021) *with* NCB Report ¶ 22.

Religiosity is the only "evidence" Barak-Corren offers to compare Louisville to the four studied states. NCB Report ¶ 20. She "observed" that Louisville "is home to the Southern Baptist Theological Seminary and some of the largest evangelical megachurches in the country." *Id.* She claims this observation proves "the high degree of religiosity in" Louisville, which causes her to "expect to observe the *Masterpiece* effect in Louisville." *Id.* ¶ 23.

But the observation appears to depend on a Wikipedia page as the only information she relies on to support her religiosity conclusions. *Compare* Wikipedia, *Religion in Louisville, Kentucky*, https://bit.ly/3srenjE (last visited Aug. 30, 2021) *with* NCB Report ¶ 20. Wikipedia is not reliable. *Advanced Mech. Servs., Inc. v. Auto-Owners Ins. Co.*, 2017 WL 3381366, at *5 (W.D. Ky. Aug. 4, 2017) (excluding expert who relied on "Wikipedia entry"). *Cf. Desai v. Charter Commc'ns, LLC*, 2018 WL 10215724, at *4 (W.D. Ky. Apr. 9, 2018) (collecting cases recognizing Wikipedia as unreliable). Anyway, if a seminary and two megachurches predicted a county's religiosity, then Los Angeles County would be as devout as Texas.[14] That just shows that Barak-Corren's testimony lacks sufficient facts and data about Louisville. *See, e.g.*, GY Report ¶¶ 16–20. Website domains don't determine devoutness.

Besides demographics, Barak-Corren has no evidence about how the *Masterpiece* Study might apply to present-day Louisville. Barak-Corren conducted her study between May and June 2018. JLS 3. She admitted that she is not an

[14] Fuller Seminary, *About Fuller*, https://bit.ly/37V9y92 (last visited Aug. 30, 2021) (describing "Fuller Seminary" in Pasadena, California as "an evangelical, multidenominational graduate institution"); Los Angeles Almanac, *Largest Protestant Christian Churches Los Angeles County*, https://bit.ly/37Va2fm (listing largest Protestant churches in Los Angeles County).

expert in wedding planning in 2021. Tr. 113:9–13. The *Masterpiece* Study also "does not measure long-term effects." GY Report ¶ 21; Tr. 176:19–24. Barak-Corren tries to extend the life of the *Masterpiece* Study by citing several other studies measuring long-term attitudinal change. Tr. 176:19–181:5. But none of those studies addressed wedding professionals. *Id.* at 181:6–12, 183:15–25. On-the-ground evidence proves the point—officials in Louisville are unaware of an increase in sexual-orientation discrimination after this Court granted Chelsey's preliminary injunction motion. *See* Exs. L, M 22:1–15, N 137:2–9.

C. Barak-Corren's testimony does not reliably apply her study's conclusions to this case.

Finally, Barak-Corren's testimony is not reliable because her conclusions rely on speculation rather than factual application. *See Gen. Elec. Co. v. Joiner*, 522 U.S. 136, 146 (1997) ("A court may conclude that there is simply too great an analytical gap between the data and the opinion proffered."). Indeed, Barak-Corren's conclusions "contain[] not just one speculation, but a string of them, whose numerosity will not permit the string to hold." *Siegel v. Fisher & Paykel Appliances Holdings Ltd.*, 746 F. Supp. 2d 845, 849 (W.D. Ky. 2010) (cleaned up).

Barak-Corren can only travel from the *Masterpiece* Study's conclusions to her conclusion here—that "granting Chelsey Nelson a religious exemption … could significantly increase the likelihood that same-sex couples … will experience discrimination" when hiring wedding professionals—by traveling a road full of inferences. NCB Report ¶ 12. But the inferences make the road impassable. The road has too many holes. The *Masterpiece* Study relies on at least eight inferences.

First, creative professionals must have been aware of the *Masterpiece* decision. *See infra* § I.A.1. *Second*, professionals must have understood the decision as granting a religious exemption. *Id.* But Barak-Corren has no evidence about professionals' exposure or understanding of *Masterpiece*. Even Barak-Corren

thinks the *Masterpiece* holding is "vague." Tr. 89:3. If the S.J.D., law professor, and author of the *Masterpiece* Study believes *Masterpiece* unclear, then it is unlikely that professionals without legal training can decipher the opinion.

Third, *Masterpiece* must have caused social norms to change. HCRCL at 46–49; JLS 37. *Fourth*, social-norm changes must have equated to non-responses. Tr. 131:6–13 (identifying non-responses as the "driver"); *infra* § I.A.4. But non-responses aren't reliable indicators of changed social norms. *See infra* § I.A.4.

Fifth, creative professionals in Louisville must have reacted the same as the *Masterpiece* Study professionals in the four other states. There's no supporting evidence. *See, e.g.*, *infra* § I.B; GY Report ¶ 20 (making this point).

Sixth, Louisville must have a different legal regime than +RFRA/+AD jurisdictions in Texas. NCB Report ¶¶ 15–16. It doesn't. *See infra* § I.A.2.

Seventh, Louisville residents must react the same to U.S. Supreme Court decisions as federal district court decisions. There's no evidence on this point.

Finally, any change in social norms must have persisted from 2018 to the present—i.e., creative professionals' attitudes towards providing services for same-sex weddings must be eternally sustainable. But the *Masterpiece* Study's "research design does not measure long-term effects." GY Report ¶ 21. *See infra* § I.B (making this point about longitudinal attitude changes) And assuming long-term attitudinal change conflicts with Barak-Corren's research on religious person's ability to reconcile conflicts between their internal beliefs and external pressure.[15]

[15] *See* Netta Barak-Corren, *Taking Conflicting Rights Seriously*, 65 Vill. L. Rev. 259, 299 (2020) (finding religious leaders "attempt to find accommodations on the ground, drawing on distinctions of sphere and role in an attempt to square traditional and liberal norms"); Netta Barak-Corren, *Beyond Dissent and Compliance: Religious Decision Makers and Secular Law*, 6 Oxford J. of L. and Religion 293, 295 (2017) (attached as Exhibit K) (summarizing how religious leaders limit conflicts between religion and law by redefining the conflict, withdrawing religious normativity from the conflict, and restraining the conflict); Netta Barak-Corren, *Does Antidiscrimination Law Influence Religious Behavior? An Empirical*

Ultimately, Barak-Corren's testimony is more speculative than testimony saying "A suggests by analogy the possibility of B, which might also apply to C, which, if we speculate about D, could eventually trigger E, so perhaps that happened here." *Tamraz*, 620 F.3d at 672. For this reason, it should be excluded.

II. Barak-Corren's testimony should be excluded as irrelevant.

Barak-Corren's testimony should also be excluded because it is irrelevant to Chelsey's constitutional or statutory claims. Expert testimony must "help the trier of fact" understand evidence or determine a fact. Fed. R. Evid. 702(a). This condition "goes primarily to relevance." *Daubert*, 509 U.S. at 591. Testimony that "does not relate to any issue in the case is not relevant and, ergo, non-helpful." *Id.*

Barak-Corren's testimony is not relevant because (A) she assumes creative professionals declined or did not respond to same-sex inquiries because of discrimination and (B) fails to show Louisville has a compelling interest in forcing Chelsey to create photographs and blogs celebrating same-sex weddings.

A. Barak-Corren's testimony is irrelevant because she assumes that all declines and non-responses are discriminatory.

Barak-Corren's testimony is irrelevant because she assumes her conclusion—that all declines or non-responses are discriminatory. But she does not consider nondiscriminatory reasons for declines, such as a creative professional's message-based objection to declining the requested service.

Take Chelsey as an example. Chelsey cannot create photographs for, write blogs about, or participate in same-sex weddings because she objects to promoting or participating in same-sex marriage. *See* Pls.' Br. in Supp. of Prelim. Inj. Mot. (MPI Br.) 4–21, ECF No. 3–1. In short, she objects to promoting certain messages,

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Examination, 67 Hastings L.J. 957, 1011 (2016) (noting "that religious people might have various ways to deal with conflicts of values other than defy the law or seek accommodations--ways that are substantially more inclusive").

not to serving certain people. *Id.* at 14–15 (making this message/status distinction). And this Court held that Chelsey's photographs and blogs are speech protected by the First Amendment. *See* Order 3–4, ECF No. 47.

Barak-Corren acknowledges that photographers "spend many hours with the couple," "take an active part in the event and are present throughout the wedding," "create the couple's wedding album," and have a "continued relationship with the couple." JLS 15. For these reasons, she found that photographers were "pickier in general about their customers …." App. 17 n.11. So like Chelsey, other photographers exercise editorial discretion over their artwork. *See* Order 14–18.

But Barak-Corren brushes all of this aside. She never explains how a message-based objection to creating photographs is like a religious exemption. *Cf.* Tr. 188:7–24 (admitting *Masterpiece* was not decided on "free speech grounds"). She also never explains how a message-based objection is equivalent to discrimination. In fact, she said the opposite at her deposition—that a cake artist "intending to provide shelf products but having a First Amendment objection to providing a custom product" would be coded as a "positive baker," i.e. non-discriminatory. *Id.* at 194:11–25. That alone proves Barak-Corren's testimony is either irrelevant or supports Chelsey's freedoms to create and participate consistent with her beliefs.

Still, Barak-Corren assumes that all negative responses were discriminatory. If this Court continues to hold that the First Amendment protects Chelsey's discretion to create photographs, write blogs, and participate in events consistent with her beliefs, then Barak-Corren's claim that *religious* exemptions lead to discrimination becomes irrelevant. *See Munoz v. Orr*, 200 F.3d 291, 301 (5th Cir. 2000) (excluding expert who assumed "promotion system discriminated against Hispanic males"); *Bickerstaff v. Vassar Coll.*, 196 F.3d 435, 450 (2d Cir. 1999) (excluding report that "ma[de] *no effort* to account for nondiscriminatory explanations for the dispar[ate]" treatment); *Raskin*, 125 F.3d at 67 (excluding age

discrimination report that "ma[de] no attempt to account for other [non-discriminatory] possible causes"). Barak-Corren cannot transform constitutional freedoms into illegal discrimination.

B. Barak-Corren's testimony is irrelevant because she fails to identify a specific problem in Louisville.

Barak-Corren's testimony should also be excluded because it is irrelevant to strict scrutiny, an element of Chelsey's claims. *Madej v. Maiden*, 951 F.3d 364, 370 (6th Cir. 2020) (courts "should consider the elements" of claim to decide relevance).

Chelsey claims that the Accommodations and Publication Provisions violate her rights guaranteed under the First Amendment and Kentucky's RFRA. Compl. ¶¶ 326–81, ECF No. 1 So Louisville must show that these provisions pass strict scrutiny—i.e., serve a compelling interest and are narrowly tailored to further that interest. MPI Br. 21–23. To show a compelling interest, Louisville must perform a "precise analysis" and justify its "interest in denying an exception" to just Chelsey. *Fulton v. City of Philadelphia*, 141 S. Ct. 1868, 1881 (2021). Barak-Corren's testimony is irrelevant to that showing because she offers no evidence that protecting Chelsey will increase sexual-orientation discrimination in Louisville.

For example, one could read the *Masterpiece* Study as justifying a need to regulate how the media reports on court decisions. As previously discussed, Barak-Corren had to evaluate how creative professionals responded to how the *media* communicated about the *Masterpiece* decision. *See* § I.A.1; HCRCL 25–27; Tr. 189:4–16. In other words, any *Masterpiece* effect comes from media portrayal, not religious exemptions. But Louisville cannot control how the media reports court cases. So Louisville has no compelling interest in refusing to recognize Chelsey's freedoms here, where any effect would be caused by the media.

And the *Masterpiece* Study cannot deny Chelsey's constitutional and statutory freedoms because it contains no relevant evidence. Barak-Corren did not

audit any professionals in Louisville. *See infra* § I.B–C. She has no evidence about how district court decisions affect social norms as compared to Supreme Court decisions. *See infra* § I.B-C. Indeed, Louisville officials are unaware of an uptick in sexual-orientation complaints since this Court granted Chelsey's preliminary injunction. *See* Exs. L, M 22:1–15, N 37:2–9. She has no evidence about how often *photographers* explicitly declined or declined by referral. GY Report ¶ 25. And she admits she is not an expert in wedding vendor behavior today. Tr. 113:9–13.

The *Masterpiece* Study did not even establish there are widespread objections to celebrating same-sex weddings. Barak-Corren sent over 1,800 emails to creative professionals inquiring about same-sex wedding services. App. 19 (noting 906 emails multiplied by two waves). She also called 177 professionals and spoke with seventy-three of them. *Id.* at 13. In almost 2,000 contacts, only one professional explicitly declined a same-sex wedding request because the professional "'only does traditional weddings.'" *Id.* at 14; Tr. 198:20–199:9. One professional also declined such a request because he or she was training to become an astronaut. Tr. 75:22–76:13. So a same-sex couple has the same chance of being declined by an astronaut preparing for launch as by someone who objects to same-sex marriage. Chelsey's fundamental rights should not be overridden by galactically unlikely events measured in unreliable ways.

Conclusion

Professor Netta Barak-Corren's testimony, including her report, her written articles, and any additional testimony she may provide should be excluded because it is speculative, unreliable, and irrelevant.

Respectfully submitted this 30th day of August, 2021.

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CERTIFICATE OF SERVICE

I hereby certify that on the 30th day of August, 2021, I electronically filed the foregoing document with the Clerk of Court using the ECF system which will send notification of such filing to all counsel of record who are registered users of the ECF system.

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UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION

Chelsey Nelson Photography LLC and **Chelsey Nelson,** Plaintiffs, **v.** **Louisville/Jefferson County Metro Government; Louisville Metro Human Relations Commission-Enforcement; Louisville Metro Human Relations Commission-Advocacy; Verná Goatley,** in her official capacity as Executive Director of the Louisville Metro Human Relations Commission-Enforcement; and **Marie Dever, Kevin Delahanty, Charles Lanier, Sr., Leslie Faust, William Sutter, Ibrahim Syed,** and **Leonard Thomas,** in their official capacities as members of the Louisville Metro Human Relations Commission-Enforcement, Defendants.	**Case No. 3:19-cv-00851-BJB-CHL** **Bryan D. Neihart's Declaration in Support of Plaintiffs' Motion to Exclude Testimony of Netta Barak-Corren**

I, Bryan D. Neihart, declare as follows:

1. I am over the age of eighteen and competent to testify, and I make this declaration based on my personal knowledge.

2. I am one of the attorneys representing Plaintiffs Chelsey Nelson Photography LLC and Chelsey Nelson in this litigation.

3. On June 30, 2021, Defendants served on Plaintiffs a Notice of Expert Disclosure, retaining Netta Barak-Corren.

4. On that same day, Defendants produced the expert report of Netta Barak-Corren. A true and correct copy of that report is attached as Exhibit A.

5. Barak-Corren's report included a law review article entitled *A License to Discriminate: The Market Response to Masterpiece Cakeshop*, which will be published in the Harvard Civil Rights-Civil Liberties Law Review. A true and correct copy of that article as produced to Plaintiffs is attached as Exhibit B.

6. Barak-Corren's report also included a law review article entitled *Religious Exemptions Increase Discrimination Towards Same–Sex Couples: Evidence from Masterpiece Cakeshop*, which will be published in the Journal of Legal Studies. A true and correct copy of that article as produced to Plaintiffs is attached as Exhibit C.

7. Barak-Corren's article attached as Exhibit B references an Appendix in footnote "*" that contains her data, methodology, and analysis. A true and correct copy of that Appendix as accessed at https://perma.cc/GF73-AHXU is attached as Exhibit D.

8. On July 13, 2021, Plaintiffs served on Defendants a Rebuttal Expert Disclosure, retaining George Yancey, Ph.D.

9. On July 26, 2021, Plaintiffs produced the rebuttal expert report of George Yancey, Ph.D. A true and correct copy of Dr. Yancey's rebuttal report is attached as Exhibit E.

10. On August 4, 2021, counsel for Plaintiffs' deposed Professor Barak-Corren. A true and correct copy of the relevant excerpts of the deposition transcript is attached as Exhibit F.

11. In addition to her own research, Barak-Corren indicated in Exhibits B and C and testified at her deposition that she relied on four relevant reports.

12. First, Barak-Corren relied on Katerina Linos & Kimberly Twist, *The Supreme Court, the Media, and Public Opinion: Comparing Experimental and*

Observational Methods, 45 J. Legal Stud. 223 (2016). A true and correct copy of the relevant excerpts of the article is attached as Exhibit G as accessed at https://www.semanticscholar.org/paper/The-Supreme-Court%2C-the-Media%2C-and-Public-Opinion%3A-Linos-Twist/a1592714cc5ffbb611e30e215d0571188339c423.

13. Second, Barak-Corren relied on Margaret E. Tankard & Elizabeth Levy Paluck, *The Effect of a Supreme Court Decision Regarding Gay Marriage on Social Norms and Personal Attitudes*, 28 Psych. Sci. 1334 (2017). A true and correct copy of the relevant excerpts of the article is attached as Exhibit H as accessed at https://spia.princeton.edu/system/files/research/documents/Paluck.pdf.

14. Third, Barak-Corren relied on Emily Kazyak & Mathew Stange, *Backlash or a Positive Response?: Public Opinion of LGB Issues After* Obergefell v. Hodges, 65 J. Homosexuality 2028 (2018). A true and correct copy of the relevant excerpts of the article is attached as Exhibit I as accessed at https://digitalcommons.unl.edu/cgi/viewcontent.cgi?article=1526&context=sociologyfacpub.

15. Fourth, Barak-Corren relied on Eugene K. Ofosu et al., *Same-Sex Marriage Legalization Associated With Reduced Implicit And Explicit Antigay Bias*, 116 Proc. Nat'l Acad. Sci. U.S. 8846 (2019). A true and correct copy of the relevant excerpts of the article is attached as Exhibit J as accessed at https://www.pnas.org/content/pnas/116/18/8846.full.pdf.

16. On April 12, 2017, Barak-Corren published an article entitled *Beyond Dissent and Compliance: Religious Decision Makers and Secular Law*, 6 Oxford J. of L. and Religion 293 (2017). A true and correct copy of the relevant excerpts of the article is attached as Exhibit K.

17. On January 25, 2021, Defendants produced Defendants' Objections and Responses to Plaintiffs' First Set of Interrogatories. A true and correct copy of the relevant excerpts of Defendants' response is attached as Exhibit L.

18. On May 26, 2021, I deposed Kendall Boyd, the Chief Equity Officer of the Louisville Metro Government and the former Executive Director of the Human Relations Commission of the Louisville Metro Government. A true and correct copy of the relevant excerpts of Mr. Boyd's deposition transcript is attached as Exhibit M.

19. On May 27, 2021, I deposed Verná Goatley, the current Executive Director of the Human Relations Commission of the Louisville Metro Government. A true and correct copy of the relevant excerpts of Ms. Goatley's deposition transcript is attached as Exhibit N.

20. A true and correct copy of the relevant excerpts of Defendants' Motion to Dismiss Plaintiff's Complaint in *U.S. Pastor Council v. City of Austin*, Case No. 1:18-cv-849-RP (W.D. Tex. Jan. 8, 2019) is attached as Exhibit O.

21. On August 26, 2021, I accessed the article Christy Chuang-Stein, *The Regression Fallacy*, 27 Drug Information J. 1213 (1993) through an account with DeepDyve. A true and correct copy of that article is attached as Exhibit P.

22. On August 26, 2021, I accessed the article Colleen Kelly & Trevor Price, Correcting for Regression to the Mean in Behavior and Ecology, 166(6) Am. Nat. 700 (2005) at https://www.semanticscholar.org/paper/Correcting-for-Regression-to-the-Mean-in-Behavior-Kelly-Price/2c771c38a72ed4695b4ce1eab24cf791726924a4. A true and correct copy of that article is attached as Exhibit Q.

Declaration Under Penalty of Perjury

I, Bryan D. Neihart, a citizen of the United States and a resident of the State of Arizona, hereby declare under penalty of perjury pursuant to 28 U.S.C. § 1746 that the foregoing is true and correct to the best of my knowledge.

Executed this 30th day of August, 2021, at Scottsdale, Arizona.

Bryan D. Neihart

EXHIBIT A

UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION

CHELSEY NELSON PHOTOGRAPHY LLC and CHELSEY NELSON, **Plaintiffs,** **v.** **LOUISVILLE/JEFFERSON COUNTY METRO GOVERNMENT, et al.,** **Defendants.**	**Case No. 3:19-cv-851-BJB**

EXPERT REPORT OF NETTA BARAK-CORREN

June 30, 2021

EXPERT WITNESS DISCLOSURE

1. I have been retained as an expert in this matter by Defendants Louisville/Jefferson County Metro Government ("Louisville Metro"), Louisville Metro Human Relations Commission – Enforcement and Louisville Metro Human Relations Commission – Advocacy, Verná Goatley, in her official capacity as Executive Director of the Louisville Metro Human Relations Commission, Marie Dever, Kevin Delahanty, Charles Lanier, Sr., Leslie Faust, William Sutter, Ibrahim Syed, and Leonard Thomas, in their official capacities as members of the Louisville Metro Human Relations Commission-Enforcement (collectively, "Defendants"), through their counsel, Kaplan Johnson Abate & Bird LLP ("Counsel"), in connection with litigation brought by Plaintiffs Chelsey Nelson Photography LLC and Chelsey Nelson (collectively, "Chelsey Nelson").

2. Chelsey Nelson is a wedding photographer in Louisville, Kentucky who filed this litigation to challenge the constitutionality of two provisions in Louisville's Fairness Ordinance (Louisville Metro Ordinance § 92.01, et seq.), which prohibits discrimination in employment, housing, and the provision of goods and services (public accommodations). Specifically, Ms. Nelson challenges the "Denial Clause," which makes it "an unlawful practice for a person to deny an individual the full and equal enjoyment of the goods, services, facilities, privileges, advantages, and accommodations of a place of public accommodation . . . on the ground of . . . sexual orientation . . .", and the "Unwelcome Clause," which makes it "an unlawful practice for a person, directly or indirectly, to publish, . . . [a] communication, notice, or advertisement, which indicates that the goods, services, facilities, privileges, advantages, and accommodations of a place of public accommodation … will be refused, withheld, or denied an individual on account of his [or her] … sexual orientation …." Louisville Metro Ordinance § 92.05(A) & (B).

3. Chelsey Nelson alleges that her religious beliefs prevent her from providing wedding photography and related services to same-sex couples and wishes to advertise her intent

to deny services to same-sex couples in violation of the Unwelcome Clause in Louisville's Fairness Ordinance. Chelsey Nelson asks the Court to grant her an exception to application of the challenged provisions in the Fairness Ordinance based on her purported rights to free speech and free exercise of religion under the First Amendment. Chelsey Nelson has also asserted claims for declaratory relief under the First Amendment's establishment clause, Fourteenth Amendment's due process clause, and Kentucky's Religious Freedom Restoration Act ("RFRA") (KRS 446.350).

4. I have been asked to provide an expert opinion regarding the anticipated effects of granting Chelsey Nelson the relief sought by her Complaint on the willingness of other wedding vendors to provide services to same-sex couples.

5. The opinions expressed in this report are based on information available to me at this time and are subject to supplementation or revision based on additional information that may emerge from depositions, additional document submissions, or other developments, including my ongoing research.

PROFESSIONAL EDUCATION AND BACKGROUND

6. I am a legal scholar and cognitive scientist, focusing on empirical and behavioral analysis of constitutional and public law, with a particular interest in conflicts of rights and the interaction between law and religion and law and social norms.

7. I received my LL.B. in Law and B.A. in Cognitive Science from the Hebrew University (Valedictorian and three-time recipient of the Albert Einstein and Rector awards). I then clerked for the Chief Justice of the Israeli Supreme Court, Hon. Dorit Beinish, and pursued doctoral studies at Harvard, graduating in 2016. During my time at Harvard, I received the Shapiro scholarship, Gammon fellowship, Sinclair Kennedy Travelling Fellowship, and the Program on Negotiation's Graduate Research Fellowship, and won the Dean's Prize in Law and Economics,

and empirical research grants from the Program on the Legal Profession, Harvard's Interfaculty Initiative on Mind, Brain, and Behavior and the Program on Negotiation's Next Generation Grant. I also served as the Inaugural Fellow of the Empirical Legal Research Group in Harvard Law School.

8. Currently, I am an Associate Professor of Law (with tenure) at the Hebrew University of Jerusalem and the Academic Director of the Center for the Study of Multiculturalism and Diversity at the Hebrew University. During 2020-2022 I am also a Nootbaar Religious Freedom Fellow at Pepperdine University School of Law.

9. My academic work was selected to important international fora, including the Stanford International Junior Faculty Forum (~5% acceptance rate) and the WZB Migration and Diversity Conference (~3% acceptance rate) and won several awards, among which Hebrew University's Birk Prize for Excellence in Legal Research, the Israeli Association of Public Law's Gorni Prize for an Outstanding Junior Scholar in Public Law, and the Menachem Goldberg, Howard Raiffa, and Fisher-Sanders Best Paper Awards.

10. My curriculum vitae, which includes any publications I have authored within the last 10 years, is attached hereto as Exhibit 1. This is my first engagement to provide expert services in connection with litigation. I am being compensated for my services in this matter at a rate of $300/hour. My compensation does not depend in any way on the outcome of this litigation or the opinions stated herein.

INFORMATION REVIEWED AND/OR RELIED UPON

11. In developing my opinions in this matter, I had discussions with Counsel and reviewed and/or relied upon the Complaint, the Court's Order and Opinion granting Chelsey Nelson's motion for preliminary injunction, Louisville's Fairness Ordinance (Louisville Metro Ordinance § 92.01, et seq.), Kentucky's RFRA (KRS 446.350), and the materials cited in my two

forthcoming papers: Netta Barak-Corren, A License to Discriminate? The Market Response to *Masterpiece Cakeshop*, 56(2), Harvard Civil Rights-Civil Liberties Law Review (forthcoming 2021), attached hereto as Exhibit 2; Netta Barak-Corren, Religious Exemptions Increase Discrimination Towards Same-sex Couples: Evidence from *Masterpiece Cakeshop*, Journal of Legal Studies (forthcoming 2021), attached hereto as Exhibit 3.

OPINION

12. Based on my work to date, I have formed the opinion that granting Chelsey Nelson a religious exemption from the application of Louisville's Fairness Ordinance in this case could significantly increase the likelihood that same-sex couples attempting to hire wedding vendors in Louisville, Kentucky will experience discrimination resulting in the denial of equal access to goods and services.

BASIS FOR MY OPINION

13. The basis for my opinion set forth above is largely the *Masterpiece*[1] experiment, described in detail in my forthcoming papers attached hereto as Exhibit 2 and Exhibit 3. As further described in these papers, I examined the impact of the *Masterpiece Cakeshop* decision on the wedding market by conducting a field experiment before and after the decision was rendered. I fielded the experiment in Iowa, North Carolina, Indiana, and Texas, states that, while sharing similar socio-economic characteristics and social and political attitudes, represent the four types of legal regimes currently in existence in the United States with respect to non-discrimination and religious freedom: regimes that prohibit discrimination on the basis of sexual orientation versus regimes that do not; and regimes that subject restrictions of religious exercise to a stringent test (primarily via state RFRAs) versus regimes that do not. My study audited wedding businesses

[1] *See Masterpiece Cakeshop, Ltd. v. Colorado Civil Rights Com'n*, 138 S. Ct. 1719 (2018).

(photographers, bakers, and florists) in those regimes both before and after the *Masterpiece* ruling and measured the impact of the decision on the willingness of wedding vendors to provide services to same-sex couples, as compared with opposite-sex couples.

14. I found that the *Masterpiece* ruling – which was decided in favor of a baker that refused to create a wedding cake for a same-sex couple – significantly reduced the agreement to serve same-sex couples as compared with opposite-sex couples, even among previously willing vendors (the "*Masterpiece* effect"). I found that the *Masterpiece* effect was stable and robust to the inclusion of all experimental covariates, including the legal regime, the type of business, and the wave of inquiry. The effect was equally strong in urban areas, which are often assumed to be particularly inclusive of same-sex couples, and did not vary with the political conservativeness of the county. However, I found that the effect varies with the religiosity of the environment, such that businesses in areas dense with religious congregations, and particularly Evangelical congregations, were more likely to change their behavior to same-sex couples post-*Masterpiece* (even as before *Masterpiece*, businesses in religiously-dense areas did not differ from other areas in how they responded to same-sex and opposite-sex couples).

15. The negative effect of *Masterpiece* was found in all legal regimes except for counties in Texas and Indiana (RFRA states) that enacted local AD rules. As I discuss in the paper attached hereto as Exhibit 3 (pages 13-18), RFRAs differ substantively across states, with Texas and Indiana belonging in a specific category of RFRAs that is not available in all states. Therefore, for the purpose of preparing this opinion, I compared Kentucky's RFRA with the RFRA laws in Texas and Indiana. Specifically, unlike the RFRAs in Indiana and Texas, which both include carve-outs for local anti-discrimination laws, Kentucky's law does not have any such carve out. Kentucky's law has only one provision, which states that "Government shall not substantially

burden a person's freedom of religion" unless the government satisfies the strict scrutiny standard. *See* KRS 446.350. The Indiana and Texas laws both contain similar language prohibiting the government from substantially burdening free exercise of religion except where the government can satisfy the strict scrutiny standard, but these laws carve-out civil rights laws. *See* Ind. Code Ann. § 34-13-9-0.7 ("This chapter does not (1) authorize a provider to refuse to offer or provide services . . . on the basis of . . . sexual orientation; (2) establish a defense to a civil action or criminal prosecution for refusal by a provider to offer or provide services . . . on the basis of . . . sexual orientation . . ."); TX CIV PRAC & REM § 110.011 (Except for religious non-profits, "this chapter does not establish or eliminate a defense to a civil action or criminal prosecution under a federal or state civil rights law."). The Indiana carve-out was added to the law after passage of Indiana's initial RFRA (which contained no such carve-out) resulted in significant public blow-back and loss of convention/events revenue.

16. As I write in the paper attached hereto as Exhibit 3 (p. 51), different RFRA designs could have different impact on discrimination, especially as these designs interact with existing or inexistent AD laws. Because of these differences between Kentucky's RFRA, on the one hand, and Texas and Indiana's RFRAs, on the other hand, with particular notice to the differences in the laws' interaction with local AD laws, I would not expect the Louisville Metro area to be immune from the statistically significant *Masterpiece* effect I observed in the *Masterpiece* experiment. That conclusion is bolstered by the high degree of religiosity of Louisville/Kentucky, which I found increased the *Masterpiece* effect in jurisdictions studied as part of the *Masterpiece* experiment.

17. To analyze religiosity, I analyzed data from the Pew Institute (Religious Landscape Data (2014), https://www.pewforum.org/religious-landscape-study/#religions) and compared that data to the same metrics observed in my paper for the jurisdictions studied as part

of the *Masterpiece* experiment.

18. The Pew Institute ranks all U.S. states by religiosity based on data from 2014. Kentucky is placed 13th with 63% of adults who are highly religious (a combined index score composed of belief in God, importance of religion, frequency of prayer, and worship attendance). This result is close to tied to that of Texas (64%) and North Carolina (65%) and higher than Indiana (54%, 22th place) and Iowa (55%, 19th place).

19. Kentucky ranks high also on importance of religion, the measure I focus on in my papers. Here, Kentucky is tied with Texas (86% who say religion is somewhat or very important in their lives), slightly above North Carolina (84%) and ahead of Iowa (79%) and Indiana (78%).

20. According to the Pew study, 49% of Kentucky's population are Evangelicals, much higher than that of all four states in my study (28%-35%), the same as Alabama and second only to Tennessee (52%). I also observed that Louisville, Kentucky is home to the Southern Baptist Theological Seminary[2] and some of the largest evangelical megachurches in the country (Southeast Christian Church and St. Stephen Baptist Church).[3]

21. According to the Pew study, Kentucky also has stronger views against homosexuality and same-sex marriage than any of the states in the *Masterpiece* experiment (44% who say that homosexuality should be discouraged, as compared to 36-37% in the states in the experiment, and 52% who are opposed or strongly opposed to same-sex marriage, as compared to 41-46% in the statues in the experiment).

22. A comparison of religiosity metrics in Kentucky and the *Masterpiece* experiment

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[2] *See*: https://www.sbts.edu/.

[3] See: https://en.wikipedia.org/wiki/Religion_in_Louisville,_Kentucky; Southeast Christian Church website (https://www.southeastchristian.org/home); St. Stephens Baptist Church website (https://www.ssclive.org/church-history/).

states is below:

Criterion	Definition	Kentucky	Iowa	North Carolina	Indiana	Texas
Importance of Religion	Religion is Somewhat/Very Important (National average: 77%)	86%	79%	84%	78%	86%
% Conservatives	(National average: 36%)	42%	41%	40%	41%	39%
% Evangelicals	(National average: 25%)	49%	28%	35%	31%	31%
Attitudes Towards Homosexuals	"Homosexuality should be discouraged" (National average: 31%)	44%	36%	36%	37%	36%
Attitudes Towards Same-Sex Marriage	Opposing/Strongly Opposing Same-Sex Marriage (National average: 39%)	52%	41%	45%	45%	46%

Source: PEW RESEARCH CENTER, RELIGIOUS LANDSCAPE STUDY (2014).

23. In closing, it is important to note that studies like the *Masterpiece* experiment can never predict future behavior with absolute certainty. The *Masterpiece* study provides evidence that judicial decisions favoring religious objectors to non-discrimination laws can have statistically significant and robust consequences, particularly in very religious areas. In the present case, due to the high degree of religiosity in the area, I would expect to observe the *Masterpiece* effect in Louisville Metro if Chelsey Nelson is granted a religious exemption to application of the Fairness Ordinance in this litigation.

Netta Barak-Corren

EXHIBIT B

56(2) HARVARD CIVIL RIGHTS-CIVIL LIBERTIES LAW REVIEW (forth. 2021)

A License to Discriminate?

The Market Response to Masterpiece Cakeshop

Netta Barak-Corren[*]

What are the consequences of religious exemptions of antidiscrimination laws? And what are the normative implications of these consequences? These questions are currently at the center of a heated debate balancing religious freedom and civil rights. Opponents of religious exemptions from antidiscrimination laws argue that granting exemptions would increase sexual orientation discrimination. Proponents of religious exemptions argue that religious objectors are a small minority and that their exemption would not meaningfully increase discrimination against same-sex couples.

The troubling aspect of this debate is that none of the parties rely on hard data. Particularly missing are data on the effects of exemptions granted in Supreme Court decisions, an issue that the Court has addressed repeatedly in recent years—and is set to do so once again this term, in *Fulton v. City of Philadelphia*.

This Article intervenes in the debate based on the results of a large-scale field experiment that measured the effect of *Masterpiece Cakeshop v. Colorado Civil Rights Commission* on the ability of same-sex couples to receive services in the

[*] Associate Professor of Law, Hebrew University of Jerusalem. For helpful comments and suggestions I thank Oren Bar Gill, Stephanie Barclay, Ittai Bar-SimanTov, Hanoch Dagan, Rick Garnett, Fred Gedicks, Noam Gidron, Michael Helfand, Ehud Kamar, Vicki Jackson, Kobi Kastiel, Amir Khoury, Ira (Chip) Lupu, Nelson Tebbe, Mila Versteeg, Eyal Zamir, and participants at the 2019 Annual Roundtable on Law and Religion at the University of Toronto, the 2019 Workshop on Behavioral Legal Studies at the Hebrew University, the 2020 Law, Society, and Psychological Science Summer Research Series, the 2020 International Forum on the Future of Constitutionalism Summer Roundtable, the 2020 Measuring Impact in Constitutional Law Conference at Chicago Law School, the Humboldt-Minerva Human Rights Under Pressure Seminar, and faculty and students in Bar Ilan University, the Hebrew University, and Tel Aviv University. Tamir Berkman provided outstanding research assistance. Responsibility for any errors is my own. The online appendix is available at https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf, *archived at* https://perma.cc/GF73-AHXU. Anonymized data and code are stored on the Open Science Framework, https://osf.io/ve5yn/?view_only=8853549b0fc248afb793ef41d4e953f8, *archived at* https://perma.cc/57DR-CFB2.

wedding market, as compared to opposite-sex couples. The field experiment revealed that after *Masterpiece Cakeshop,* vendors were less willing to provide wedding services to same-sex couples than to opposite-sex couples. This trend was true even for vendors that provided services to same-sex couples prior to the *Masterpiece Cakeshop* decision. Following *Masterpiece Cakeshop,* the odds that same-sex couples would experience discrimination from wedding vendors are estimated to be between 61% and 85%.

These results have several implications for the debate on religious exemptions. First, they discredit the argument that the effects of religious exemptions are negligible, making clear that exemptions *will* promote further discrimination. Second, the results complicate the conventional portrait of religious objection as fixed (and therefore unyielding to change), showing instead that the demand for discrimination is elastic and shaped by social constructions, even without coercion or sanctions. These negative effects of *Masterpiece Cakeshop* bear on both litigation—showing that antidiscrimination laws are necessary to further states' compelling interest in securing equality—and in legislation—providing guidance for legislatures on whether and how to enact religious exemptions from antidiscrimination laws.

Finally, the troubling consequences of *Masterpiece Cakeshop* require the Supreme Court to proceed with great care as it sets out to decide *Fulton v. City of Philadelphia* and any other future cases raising ostensible conflicts between religion and anti-discrimination law. However the Court decides to resolve the constitutional issue at hand, it must take into account that even a deliberately narrow and case-specific exemption might have a significant negative impact on the market and its customers.

INTRODUCTION

The conflict between religious liberty and marriage equality is escalating. This term, the Supreme Court is set to decide *Fulton v. City of Philadelphia*,[1] a case which raises the constitutionality of an antidiscrimination rule that denies religious exemptions for state contractors who refuse to serve same-sex couples. Only two years ago, the Court addressed the conflict in the private market context, in *Masterpiece Cakeshop, Ltd. v. Colorado Civil Rights Commission*, ruling for a baker who refused to create a wedding cake

[1] Fulton v. City of Philadelphia, 922 F.3d 140, 146–47 (3rd Cir. 2019), *cert. granted*, 140 S. Ct. 1104 (2020) (No. 19-123).

for a same-sex couple.[2] Shortly after the decision, the Court vacated and remanded two similar cases, one involving a florist who would not create flower arrangements for a same-sex wedding[3] and another case involving wedding cakes.[4] An impressive number of similar cases have been making their way through the courts in recent years, involving photographers and video artists,[5] a web-designer,[6] a t-shirt store,[7] a custom wedding invitation studio,[8] and a bed and breakfast[9]—all of whom object to serving same-sex couples and seek exemptions from antidiscrimination laws.

This state of affairs has caused anxiety and controversy among citizens, lawmakers, and legal scholars. All of these groups are concerned with potential on-the-ground consequences of religious exemptions from antidiscrimination laws. Opponents of religious exemptions warn that granting exemptions will escalate the number and significance of faith claims and could expand sexual orientation discrimination to all facets of public life.[10] Proponents of religious exemptions reject these concerns as factual nonsense, arguing that religious objectors are a negligible minority in a society growing ever more affirming of marriage equality, and that exempting religious objectors will not exacerbate discrimination against same-sex couples.[11]

The relationship between religious exemptions from antidiscrimination law and the actual consequences for same-sex couples and for religious objectors is thus a central question. Yet there is almost no evidence that could

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[2] Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm'n, 138 S. Ct. 1719, 1720 (2018).

[3] Arlene's Flowers, Inc. v. Washington, 138 S. Ct. 2671 (2018), *remanded to* State v. Arlene's Flowers, Inc., 441 P.3d 1203, 1209 (Wash. 2019). A second petition for certiorari was filed in 2019, and was not decided by the time this article went to press. *See* Petition for Writ of Certiorari, Arlene's Flowers, Inc. v. Washington, No. 19-333 (U.S. Sept. 11, 2019).

[4] Klein v. Or. Bureau of Lab. & Indus., 139 S. Ct. 2713 (2019).

[5] Telescope Media Grp. v. Lucero, 936 F.3d 740, 747 (8th Cir. 2019).

[6] 303 Creative L.L.C. v. Elenis, 405 F. Supp. 3d 907, 912 (D. Colo. 2019), *appeal docketed*, No. 19-01413 (10th Cir. argued Nov. 16, 2020).

[7] Lexington-Fayette Urban Cnty. Hum. Rts. Comm'n v. Hands On Originals, 592 S.W.3d 291, 294 (Ky. 2019).

[8] Brush & Nib Studio v. City of Phoenix, 448 P.3d 890, 897 (Ariz. 2019).

[9] Aloha Bed & Breakfast v. Cervelli, 415 P.3d 919, 923 (Haw. Ct. App. 2018), *cert. denied*, 139 S. Ct. 1319 (2019).

[10] *See infra* notes 66–69.

[11] *See infra* notes 76–82.

help clarify which of the contradictory factual premises is actually true. Such evidence is required to inform legislators debating whether to enact religious exemptions, and courts deliberating whether to grant such exemptions. Underscoring the importance of the consequentialist consideration, Justice Kennedy asked the U.S. Solicitor General[12] during the *Masterpiece Cakeshop* oral arguments, "what would the government's position be if . . . the baker prevails in this case, and then bakers all over the country received urgent requests: Please do not bake cakes for gay weddings. And more and more bakers began to comply. Would the government feel vindicated in its position that it now submits to us?"[13] The Solicitor General responded that this would make the case for antidiscrimination "much stronger" because states would be able to show "that the application of the law is narrowly tailored to the government's interest in ensuring access [to public accommodations]."[14] Justice Kennedy was not alone on the bench in considering the consequences of religious exemptions as the key for the decision to grant them. From *Employment Division v. Smith*[15] to *Burwell v. Hobby Lobby Stores, Inc.*,[16] the Court has consistently cited consequentialist concerns (or lack thereof) in rejecting (or granting) requested religious exemptions.

This article contributes to the consequentialist debate on religious exemptions by studying, for the first time, the effects of religious exemptions on sexual orientation discrimination. Part I begins with surveying the relevant legal background on the tension between marriage equality and religious liberty. It addresses the evolution of the conflict and the legislative patchwork of protections across the nation, where some jurisdictions prohibit sexual orientation discrimination in public accommodations (dubbed here "AD Law" regimes, for convenience purposes) and others do not; where some

[12] The U.S. Solicitor General argued as amicus curiae supporting the baker in *Masterpiece Cakeshop*. *See* Transcript of Oral Argument at 2, Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm'n, 138 S. Ct. 1719 (2018) (No. 16-111).

[13] *Id.* at 45–46.

[14] *Id.* at 46.

[15] 494 U.S. 872, 879 (1990) (noting the concern that permitting an exemption is "in effect to permit every citizen to become a law unto himself") (quoting Reynolds v. United States, 98 U.S. 145, 167 (1879)).

[16] 573 U.S. 682, 692–93 (2014) ("[O]ur holding is very specific. . . . [W]e certainly do not hold or suggest that 'RFRA demands accommodation of a for-profit corporation's religious beliefs no matter the impact that accommodation may have' . . . . The effect of the HHS-created accommodation on the women employed by Hobby Lobby and the other companies involved in these cases would be precisely zero.").

jurisdictions facilitate religious exemptions via a Religious Freedom Restoration Act ("RFRA") and others do not.[17] This varied setting provides the context for the debate about religious exemptions and for *Masterpiece Cakeshop* itself. Part I concludes with analyzing the opposing consequentialist arguments about religious exemptions, exposing their lack of evidentiary foundations, and the implications of these omissions.

Part II then describes the large-scale field experiment designed to elucidate and inform the consequentialist debate by measuring the impact of the *Masterpiece Cakeshop* decision on sexual orientation discrimination in the wedding vendor market. An extended analysis of the experiment is reported in a separate methodological paper.[18] Wedding vendors (bakers, photographers, and florists; N = 1,155) were sampled from the four types of regimes currently in existence (those with or without antidiscrimination laws; and those with or without religious freedom laws). Each business was contacted via email by four different couples: two shortly before and two shortly after the *Masterpiece Cakeshop* ruling; in each period, one of the test couples was a same-sex couple and the other was an opposite-sex couple. The total dataset includes four observations per business, allowing for both within- and across-businesses comparisons. The question of interest was whether businesses agreed to provide the requested service to the couples.

What were the results of the field experiment? In brief, the field experiment demonstrated that the Court's decision in *Masterpiece Cakeshop* significantly reduced the willingness of businesses to serve same-sex couples: while 63.6% were willing to serve same-sex couples before *Masterpiece Cakeshop*, only 49.2% were so willing after the decision was rendered (a 14.4 percentage-point gap, or ~23 percent decrease in favorable responses). Zooming in on businesses that, prior to *Masterpiece Cakeshop*, responded positively to same-sex couples, I find that many of these businesses discriminate between opposite-sex and same-sex couples after *Masterpiece Cakeshop*: previously "gay-friendly" businesses that were

[17] These are broad distinctions. Additional nuances are discussed *infra* Part II.B.

[18] Netta Barak-Corren, *Religious Exemptions Increase Discrimination Towards Same-Sex Couples: Evidence from* Masterpiece Cakeshop, J. LEGAL STUD. (forthcoming 2021). Parts of the current article have been adapted from there. The research was approved by the Hebrew University IRB.

randomly contacted by opposite-sex or same-sex couples responded less favorably to same-sex couples than opposite-sex couples (75.5% vs. 66.3%, a 9 percentage-point gap, or 12 percent fewer favorable responses) after the decision was rendered. This effect is not an artifact of the experiment itself, as it is identically found in the "control" group of businesses that were contacted for the first time after *Masterpiece Cakeshop*. Probing into the differences between the four regime types, I find that the negative *Masterpiece Cakeshop* effect appears in all regimes, including regimes without AD laws and regimes without RFRAs, except for those that enacted *both* an AD law and a RFRA. The effect is robust, and remains so when including analyses that control for county-level conservativeness and analyses limited to businesses located in big cities (where, it is often argued, there is considerably less of a discrimination problem). However, the effect of *Masterpiece Cakeshop* is significantly more pronounced in religious environments, as proxied by the density of religious congregations in the county where the business is located.

A back-of-the-envelope calculation demonstrates the broader implications of these results. Provided that couples of all identities typically contract with about ten types of vendors in the process of organizing a wedding (reception venues, wedding planners, bakers, florists, photographers, videographers, bridal/groom salons, jewelers, DJs, and calligraphers—a partial list), that they often inquire with several vendors from each category, and that the average risk of experiencing discrimination per vendor post-*Masterpiece Cakeshop* is about 9%, I estimate the aggregate risk of experiencing at least one instance of discrimination ranges between 61% and 85% for same-sex couples. This means that across the observed differences between businesses, legal regimes, and religious environments, *Masterpiece Cakeshop* had the general effect of exposing same-sex couples to a substantial and heightened risk of discrimination while planning a wedding.

With this novel evidence, Part III returns to the normative debate and considers the implications of the law of religious exemptions. First, the results of the *Masterpiece Cakeshop* experiment discredit the argument that the effect of religious exemptions is negligible and that exemptions will not promote discrimination. Instead, what the *Masterpiece Cakeshop* experiment

shows is that even an intentionally narrow and case-specific exemption can have a substantial impact on an industry and its customers. Second, the results complicate the conventional portrait of religious objection as fixed, showing instead that the demand for discrimination is elastic and shaped by social constructions, even without coercion or sanctions. Third, this evidence makes clear that states and localities have a compelling interest in passing and enforcing anti-discrimination laws, and that such laws are narrowly tailored to that interest. Antidiscrimination laws thus satisfy strict scrutiny (and lower thresholds of judicial review, where applicable).

At the same time, the documented variation between legal regimes tentatively suggests that there is still room for legislatures to explore ways to protect *both* marriage equality and religious freedom, without necessarily increasing discrimination. I suggest specific ways in which legislators could improve the regulation of religion-equality conflicts, by actively seeking to ground policy in data. In particular, I argue that new laws should be experimentally pre-tested to inform lawmakers as to the likely consequences. I demonstrate how such pre-testing could be performed and I explain its advantages.

As is true for any empirical work, this article does not purport to exhaust or conclude the debate about the consequences of religious exemptions. Indeed, this would be impossible. The article is a snapshot of reality at a specific point in time and place and is limited in what such a snapshot can reveal about society—particularly when it comes to complex phenomena such as the relationship between law and behavior. Notwithstanding these important limitations, the centrality of empirical assumptions to the resolution of the debates in constitutional law requires us to grapple with the empirical questions rather than treating them as axioms. The current debate illustrates this need well. Opponents and proponents of religious exemptions rely on conflicting assumptions regarding the consequences of exemptions, largely talking past each other. While there is no assurance that the opposing camps will digest empirical evidence willingly and without bias, there is always hope that at least some will (indeed, this is the underlying premise of all scientific work). At the very least, disagreements about the relevance of the data could increase the sophistication of legal arguments and generate new questions for debate and empirical investigation. For now, the troubling

effects of *Masterpiece Cakeshop* suggest that the Supreme Court should question empirical arguments that do not base themselves on relevant data and should carefully consider the probable consequences of its impending decision in *Fulton v. City of Philadelphia* and in any other religion-equality conflict that will come before the Court in the future.

I. THE TENSION BETWEEN MARRIAGE EQUALITY AND RELIGIOUS LIBERTY

The tension between sexual orientation equality and religious liberty has been present from the inception of the movement for marriage equality. When Massachusetts became the first State to recognize same-sex marriage in 2004, the Supreme Judicial Court recognized the possibility of such a conflict when it asserted that its "decision [to uphold same-sex marriage laws] in no way limits the rights of individuals to refuse to marry persons of the same-sex for religious or any other reasons. It in no way limits the personal freedom to disapprove of, or to encourage others to disapprove of, same-sex marriage."[19] Similarly, when the Iowa Supreme Court recognized same-sex marriage—the fourth high court to follow this route, after Massachusetts, California and Connecticut—it stated that "[r]eligious doctrine and views contrary to this principle of law are unaffected, and people can continue to associate with the religion that best reflects their views."[20] In 2015, when the U.S. Supreme Court legalized same-sex marriage across the nation in *Obergefell v. Hodges*, it emphasized that "religions, and those who adhere to religious doctrines, may continue to advocate with utmost, sincere conviction that, by divine precepts, same-sex marriage should not be condoned."[21]

Other courts expressed reservations about the possibility of mitigating the tension between religion and sexual orientation equality. When the Connecticut Supreme Court recognized same-sex marriage in 2008, it dedicated a lengthy paragraph to describe the religious condemnation of homosexuality and to present it as one of the roots of discrimination towards

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[19] Goodridge v. Dep't of Pub. Health, 798 N.E.2d 941, 965 n.29 (Mass. 2003).

[20] Varnum v. Brien, 763 N.W.2d 862, 906 (Iowa 2009).

[21] 576 U.S. 644, 679 (2015). Notably, any reference to a potential tension between religious liberty and marriage equality was omitted from a previous marriage equality decision, *United States v. Windsor*, in which the Court struck down the Defense of Marriage Act, a federal law defining marriage as an act between a man and a woman. 570 U.S. 744, 745 (2013).

gay people in society.[22] The court then observed that "[f]eelings and beliefs predicated on such profound religious and moral principles are likely to be enduring, and persons and groups adhering to those views undoubtedly will continue to exert influence over public policy makers."[23] Several years later, Justice Alito dissented from the United States Supreme Court's decision in *Obergefell* with the opposite prediction, expressing concern that "those who cling to old beliefs will be able to whisper their thoughts in the recesses of their homes, but if they repeat those views in public, they will risk being labeled as bigots and treated as such by governments, employers, and schools."[24]

Whether mitigating this tension is possible or not remains to be seen. What is clearly evident, however, is that religion-equality conflicts are rapidly gaining legal momentum and public attention. As the primary origin of these conflicts has been state law, it is necessary to understand the variation between states to assess the background against which religious exemptions are debated.

A. Antidiscrimination Laws and Claims for Religious Exemptions

At present, federal law does not prohibit discrimination on the basis of sexual orientation in public accommodations. Title II of the Civil Rights Act does not prohibit discrimination on the basis of either sex or sexual orientation;[25] even if it did, it limits "public accommodation" to hotels, restaurants, gas stations, and places of exhibition or entertainment.[26] This definition does not include most of the businesses currently refusing service to same-sex couples, in particular most wedding vendors.[27]

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[22] Kerrigan v. Comm'r of Pub. Health, 957 A.2d 407, 444–45 (Conn. 2008).

[23] *Id.* at 445.

[24] *Obergefell*, 576 U.S. at 741 (Alito, J., dissenting).

[25] 42 U.S.C. § 2000a(a). This omission is in contrast to the prohibition on discrimination on the basis of "sex" in employment in Title VII of the Civil Rights Act, a provision that has been interpreted as also covering sexual orientation discrimination. Bostock v. Clayton Cnty., 140 S. Ct. 1731, 1754 (2020).

[26] 42 U.S.C. § 2000a(b).

[27] *See, e.g.*, State v. Arlene's Flowers, Inc., 441 P.3d 1203, 1209 (Wash. 2019); Telescope Media Grp. v. Lucero, 936 F.3d 740, 747 (8th Cir. 2019); 303 Creative L.L.C. v. Elenis, 405 F. Supp. 3d 907, 912 (D. Colo. 2019), *appeal docketed*, No. 19-01413 (10th Cir. argued Nov. 16, 2020).

Acting to fill the void, twenty-two states, the District of Columbia, and numerous local governments passed legislation prohibiting discrimination based on sexual orientation and/or gender identity in public accommodations ("AD states," see Figure 1).[28] Most of these laws permit no religious exemptions,[29] and their definitions of "public accommodations" are generally much broader than that of federal law,[30] covering any business open to the public. These laws are the underpinnings of the lawsuits against wedding vendors that refused to provide service to same-sex commitment ceremonies and weddings, citing religious reasons.[31] Concomitantly, and particularly after the recognition of marriage equality in *Obergefell*, conservative faith groups began calling for religious exemptions from AD laws.[32] On the legislative front, some states took steps to advance these calls.[33] In courts,

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[28] HUM. RTS. CAMPAIGN FOUND., 2020 STATE EQUALITY INDEX 14 (2020), https://hrc-prod-requests.s3-us-west-2.amazonaws.com/HRC-SEI20-report-Update-022321-FInal.pdf?mtime=20210322114741&focal=none, *archived at* https://perma.cc/YG5Y-35Q2. Note that Figure 1 presents the state of the law at the time of the study in 2018, before Virginia prohibited discrimination on the basis of sexual orientation in public accommodations.

[29] Many states provide exemptions for churches and affiliated religious organizations, but these exemptions mostly do not extend to private for-profit businesses. *See* Lucien J. Dhooge, *Public Accommodation Statutes and Sexual Orientation: Should There Be a Religious Exemption for Secular Businesses?*, 21 WM. & MARY J. WOMEN & L. 319, 344 (2015).

[30] For example, IOWA CODE § 216.2.13(a) defines "public accommodation" as "each and every place, establishment, or facility of whatever kind, nature, or class that caters or offers services, facilities, or goods for a fee or charge to nonmembers of any organization or association utilizing the place, establishment, or facility."

[31] Such lawsuits against wedding vendors have been brought in various states. *See* Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm'n, 138 S. Ct. 1719 (2018) (in Colorado); *Arlene's Flowers*, 441 P.3d at 1209 (in Washington); *Telescope Media Grp.*, 936 F.3d at 740 (in Minnesota); Brush & Nib Studio v. City of Phoenix, 448 P.3d 890 (Ariz. 2019) (in Arizona); Cervelli v. Aloha Bed & Breakfast, 415 P.3d 919 (Haw. Ct. App. 2018), *cert. denied*, 139 S. Ct. 1319 (2019) (in Hawaii); Klein v. Or. Bureau of Lab. & Indus., 410 P.3d 1051 (Or. Ct. App. 2017) (in Oregon); Elane Photography, L.L.C. v. Willock, 309 P.3d 53 (N.M. 2013) (in New Mexico).

[32] Erik Eckholm, *Conservative Lawmakers and Faith Groups Seek Exemptions After Same-Sex Ruling*, N.Y. TIMES (June 26, 2015), https://www nytimes.com/2015/06/27/us/conservative-lawmakers-and-faith-groups-seek-exemptions-after-same-sex-ruling.html, *archived at* https://perma.cc/N48H-G6WN; *Religious groups react to Supreme Court ruling on same-sex marriage*, TAMPA BAY TIMES (June 26, 2015), https://www.tampabay.com/news/courts/religious-groups-react-to-supreme-court-ruling-on-same-sex-marriage/2235233/, *archived at* https://perma.cc/5X7P-X27S.

[33] *See infra* Part I.C.

most wedding-vendor cases ended in defeat for the vendors.[34]

Masterpiece Cakeshop was the first case in which the Supreme Court granted a petition for certiorari on the question of whether laws forbidding discrimination on the basis of sexuality violate religious freedom.[35] Arising under Colorado's AD law, the case presented a conflict between Jack Phillips—the owner of Masterpiece Cakeshop—and Charlie Craig and David Mullins, a same-sex couple who attempted to purchase a cake from Phillips, unaware of Phillips' beliefs. Phillips declined to make the cake, citing his objection to same-sex marriages. The parties dispute whether Phillips offered to sell other products at his store to the couple: Phillips argues that he "offered to make any other cake for them,"[36] but the couple argues that Phillips said that "while the bakery would sell baked goods to gay and lesbian customers for other purposes, it would not sell them baked goods for weddings"[37] and that "the bakery has repeatedly refused to provide any baked goods . . . for wedding receptions or commitment ceremonies of same-sex couples."[38]

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[34] *See, e.g.*, *Arlene's Flowers*, 441 P.3d at 1237; *Aloha Bed & Breakfast*, 415 P.3d at 923; *Elane Photography*, 309 P.3d at 59; 303 Creative L.L.C. v. Elenis, 405 F. Supp. 3d 907, 912 (D. Colo. 2019). *Contra Brush & Nib Studio*, 448 P.3d at 926 (holding that Phoenix's public accommodations ordinance unconstitutionally compels speech and violates the vendor's free exercise of religion).

[35] Before *Masterpiece Cakeshop*, the Supreme Court denied certiorari in *Elane Photography, L.L.C. v. Willock*, 572 U.S. 1046 (2014). After *Masterpiece Cakeshop*, the Supreme Court granted certiorari in *Arlene's Flowers, Inc. v. Washington*, 138 S. Ct. 2671 (2018), and *Klein v. Or. Bureau of Lab. & Indus.*, 139 S. Ct. 2713 (2019), vacating and remanding both cases for further consideration in light of *Masterpiece Cakeshop*. *Arlene's Flowers*, 138 S. Ct. at 2671; *Klein*, 139 S. Ct. at 2713.

[36] Petition for Writ of Certiorari at 6, *Masterpiece Cakeshop*, 138 S. Ct. 1719 (No. 16-111).

[37] Brief for Respondents Charlie Craig & David Mullins at 4, *Masterpiece Cakeshop*, 138 S. Ct. 1719 (No. 16-111).

[38] *Id*. at 1, 4–5.

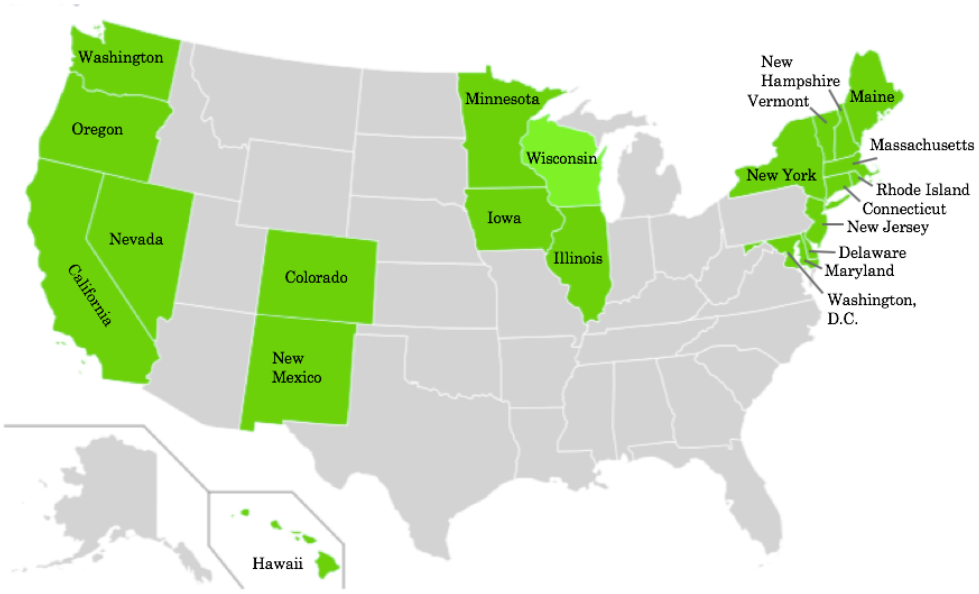

Figure 1. States prohibiting sexual orientation and gender identity discrimination in public accommodations as of 2018.[39]

The Colorado Civil Rights Commission, the administrative body that adjudicates claims under the Colorado Anti-Discrimination Act, found that Phillips discriminated against the couple based on their sexual orientation. During the proceedings, a member of the Commission stated that "to me it is one of the most despicable pieces of rhetoric that people can use to—to use their religion to hurt others."[40] Ultimately, these and related comments were among the primary reasons cited by the Supreme Court when it reversed and invalidated the Commission's decision. The Court noted that the Commission failed to treat Phillips neutrally and fairly and instead showed

[39] HUM. RTS. CAMPAIGN FOUND., *supra* note 28, at 14. Wisconsin prohibits only sexual orientation discrimination. *Id.* The map does not include local governments that prohibit discrimination within their boundaries. Note that Figure 1 presents the state of the law at the time of the study in 2018, before Virginia prohibited discrimination on the basis of sexual orientation in public accommodations.

[40] *Masterpiece Cakeshop*, 138 S. Ct. at 1729.

unconstitutional religious hostility towards his beliefs.[41] In a concurrence joined by Justice Gorsuch, Justice Thomas opined that Phillips should have also prevailed on free speech grounds, stating that creating and designing custom wedding cakes is a form of expressive conduct.[42]

While Phillips won the case on free exercise grounds, the decision also affirmed the need for AD laws to protect against sexual orientation discrimination in the marketplace. The majority acknowledged that "if [religious] exception[s] were not confined, then a long list of persons who provide goods and services for marriages and weddings might refuse to do so for gay persons, thus resulting in a community-wide stigma inconsistent with the history and dynamics of civil rights laws."[43] For this reason, the Court did not rule out the possibility that Colorado could eventually rule against Phillips and similarly situated vendors on the basis of its AD law if the state guaranteed a neutral and respectful process to all parties. More generally, the majority's opinion did not expressly resolve the bigger issue of the relationship between religious liberty and sexual orientation equality.

B. Religious Freedom Laws and Claims for Religious Exemptions

Thus far, I have surveyed the tension between marriage equality and religious liberty from the standpoint of AD legislation. Another type of legislation that bears on the legal status of religion-equality conflicts are Religious Freedom Restoration Acts ("RFRAs").

Congress enacted the first national RFRA in response to *Employment Division v. Smith*, which held that neutral laws of general applicability (i.e., those that do not intentionally target religion) are constitutional even if they substantially burden the free exercise of religion.[44] Before *Smith*, one of the

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[41] *Id.* at 1723. The Court also found another indication of hostility in "the difference in treatment between Phillips' case and the cases of other bakers who objected to a requested cake on the basis of conscience and prevailed before the Commission." *Id.* at 1730. These other bakers refused to create cakes with images that conveyed disapproval of same-sex marriage, and the Commission found their refusal legal because the bakers deemed the messages offensive. *Id.* The Court criticized this differential treatment as a showing of hostility towards Phillips' faith. *See id.*

[42] *Id.* at 1742 (Thomas, J., concurring).

[43] *Id*. at 1727.

[44] 494 U.S. 872, 879 (1990).

tests used by the Court to review substantial burdens on religious freedom required that such burdens be the least restrictive means of serving a compelling government interest (a test known as "strict scrutiny").[45] Congress sought to enact that standard through the national RFRA, which provided that the "Government shall not substantially burden a person's exercise of religion even if the burden results from a rule of general applicability" unless the burden serves "a compelling government interest" and is "the least restrictive means" to further that interest.[46] But the Supreme Court limited the national RFRA's scope to the federal government and invalidated it as applied to the states.[47] Twenty-one states responded by enacting state-level RFRAs to ensure that their governments are subject to the same high level of scrutiny as the federal government (see Figure 2).[48] In ten additional states, courts have interpreted their constitutions to require strict scrutiny.[49]

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[45] *See* Sherbert v. Verner, 374 U.S. 398, 403 (1963); Wisconsin v. Yoder, 406 U.S. 205, 219 (1972). Strict scrutiny, however, was not evenly or consistently applied before *Smith*, and the Court sidestepped it in a series of cases. *See* O'Lone v. Est. of Shabazz, 482 U.S. 342, 349 (1987) (declining to apply strict scrutiny to prison policy); Goldman v. Weinberger, 475 U.S. 503, 507 (1986) (declining to apply strict scrutiny to military policy); United States v. Lee, 455 U.S. 252, 261 (1982) (declining to exempt Amish employers from social security policy); Bob Jones Univ. v. United States, 461 U.S. 574, 604 (1983) (declining to reverse the denial of tax benefits of a university, stating that government's interest in eradicating racial discrimination outweighs the burden on the university's religious exercise). *See generally* Ira C. Lupu, Hobby Lobby *and the Dubious Enterprise of Religious Exemptions*, 38 HARV. J. L. & GENDER 35, 51–53 (2015) (exploring the court's treatment of free exercise claims pre-*Smith*).

[46] 42 U.S.C. § 2000bb to 2000bb-4 (1993), *invalidated by* City of Boerne v. Flores, 521 U.S. 507 (1997) (finding RFRA unconstitutional as applied to the states).

[47] *City of Boerne*, 521 U.S. at 532. Congress amended RFRA to reflect the holding and removed the words "a State, or a subdivision of a State" from the definition of "government" in the law. *See* 42 USC § 2000bb-2.

[48] *State Religious Freedom Restoration Acts*, NAT'L CONF. STATE LEGISLATURES (May 4, 2017), http://www.ncsl.org/research/civil-and-criminal-justice/state-rfra-statutes.aspx, *archived at* https://perma.cc/QV3E-F67V [hereinafter NCSL] (providing an up-to-date survey of all RFRAs).

[49] Eugene Volokh, *1A. What Is the Religious Freedom Restoration Act?*, VOLOKH CONSPIRACY (Dec. 2, 2013), http://volokh.com/2013/12/02/1a-religious-freedom-restoration-act/, *archived at* https://perma.cc/DXZ6-9PLN (surveying state RFRAs and state interpretations of their constitutions to require strict scrutiny; since then, AR, IN, and MI also enacted RFRAs).

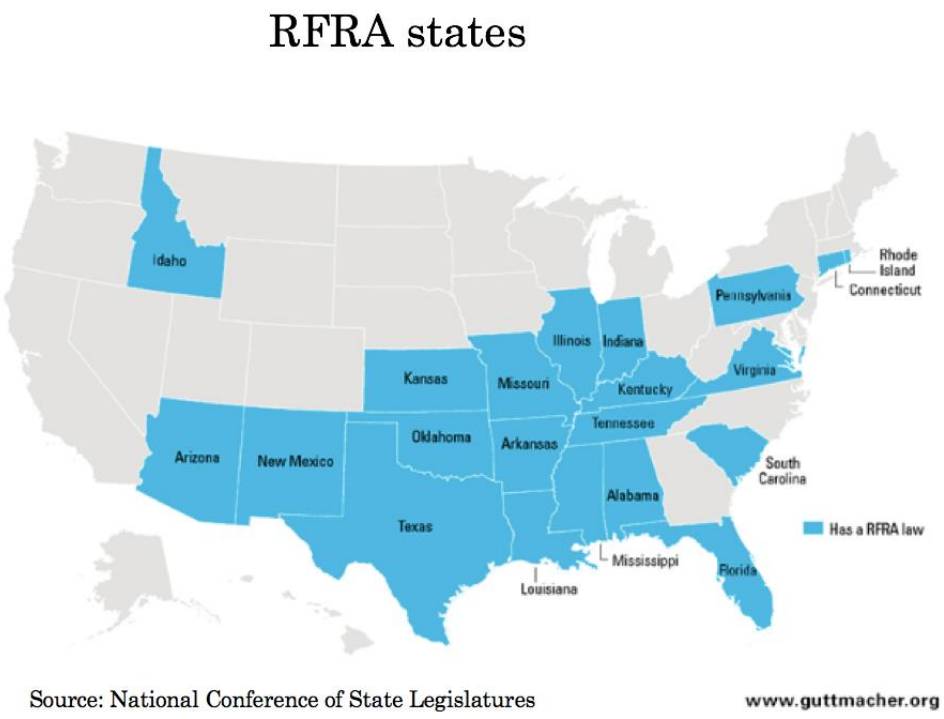

Figure 2. States that have enacted Religious Freedom Restoration Acts as of 2018. Note: The map does not include states that interpret their constitutions to require a RFRA-like protection of religious freedom: AK, MA, ME, MI, MN, MT, NC, OH, WA, and WI.[50]

While RFRAs do not provide absolute guarantees of religious exemptions, conservative legislators in RFRA-less states began pushing for the enactment of RFRAs as a shield (or in some cases, a sword) against potential duties to recognize the validity of same-sex marriage. Mississippi passed a RFRA in 2014; Indiana and Arkansas in 2015.[51] Yet in other states, such as Iowa and Georgia, RFRA bills failed due to public concerns about their implications for LGBTQ rights and fears of commercial boycotts.[52] In the process, RFRAs came to be viewed as the legislative opposite of AD laws.[53]

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[50] *Id*.

[51] NCSL, *supra* note 48.

[52] Kathleen Foody, *Ga. lawmakers leave without vote on religious freedom bill*, WASH. TIMES (Apr. 3, 2015), https://www.washingtontimes.com/news/2015/apr/3/religious-freedom-measure-focus-of-ga-lawmakers-la/, *archived at* https://perma.cc/77XQ-AUDA.

[53] *See*, *e.g.*, David Ferguson, *LGBT rights amendment proves to be 'poison pill' for*

C. The Implications of the "Legislative Mismatch"

Figures 1 and 2 show that the distribution of AD laws and RFRAs across states is what Professor Lupu has termed a "legislative mismatch" with a relatively narrow overlap. As Professor Lupu notes, the overlap consists of four states that enacted both laws (Connecticut, Illinois, New Mexico, and Rhode Island), a maximum of seven states that have both AD laws and extended protections on religious freedom in their constitutions but no RFRA (Maine, Massachusetts, Minnesota, New York, Vermont, Washington, and Wisconsin),[54] and a considerable number of local governments in RFRA states that enacted municipal AD laws. This last category includes a number of major cities in conservative states, such as Dallas, Texas, Indianapolis, Indiana, Phoenix, Arizona, and Atlanta, Georgia.[55]

The legal variation that results from the "legislative mismatch" potentially entails very different outcomes for otherwise identical cases. Imagine a photographer refusing to take the engagement photos of a same-

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Georgia's 'religious freedom' bill, RAWSTORY (Mar. 27, 2015), https://www rawstory.com/2015/03/lgbt-rights-amendment-proves-to-be-poison-pill-for-georgias-religious-freedom-bill/, *archived at* https://perma.cc/39CX-J8J6 (reporting how the passage of an amendment preventing the bill from affecting the state's civil rights laws collapsed support of the bill).

[54] Ira C. Lupu, *Moving Targets:* Obergefell, Hobby Lobby, *and the Future of LGBT Rights*, 7 ALA. C.R. & C.L.L. REV. 1, 45–46 (2015) (classifying states into categories, and since which, no new laws have been enacted to change this classification). Some uncertainty exists as to which states have interpreted their constitutions to require a RFRA-like standard of review. Volokh, *supra* note 49, classifies Hawaii and Vermont as states where courts have explicitly noted uncertainty about whether their constitution entails such a standard, and declined to resolve it, and New York as a state with weak intermediate review. In Hawaii, that uncertainty was recently noted in a case of a wedding service refusal. Cervelli v. Aloha Bed & Breakfast, 415 P.3d 919, 934 (Haw. Ct. App. 2018) ("We need not decide whether a higher level of scrutiny should be applied to a free exercise claim under the Hawai'i constitution . . . because we conclude that [Hawaii AD law] satisfies even strict scrutiny as applied to Aloha B&B's free exercise claim.").

[55] Lupu, *supra* note 54, at 46; *Texas' Equality Profile*, MOVEMENT ADVANCEMENT PROJECT, https://www.lgbtmap.org/equality_maps/profile_state/TX, *archived at* https://perma.cc/BC78-VFGU (last visited Apr. 3, 2021); *Indiana's Equality Profile,* MOVEMENT ADVANCEMENT PROJECT, https://www.lgbtmap.org/equality maps/profile state/IN, *archived at* https://perma.cc/A77G-U2A9 (last visited Apr. 3, 2021). No local government in an AD state has enacted a municipal RFRA thus far. Local laws are typically enforceable by complaint to a city agency.

sex couple. In solely AD states, a discrimination claim will likely result in victory for the couple.[56] In solely RFRA states, such claim will likely fail. In states that enacted neither type of law (e.g., North Carolina), the claim's fate will likely be similar to RFRA states, if only because there is no vehicle to bring an antidiscrimination claim forward. And in the overlap category, where both sexual orientation and religious freedom are afforded legislative protections, the claim's fate would depend on how courts interpret the relationship between the two laws, including their potential application of strict scrutiny to the state's AD law.

Although one may assume that the conflict is strongest in the overlap states, it is not necessarily the case. For example, the four states with both AD laws and RFRAs construed their RFRAs to apply only to government agencies, excluding legislatures and courts, or limited relief to be only against the government, excluding private parties.[57] This structure led the New Mexico Supreme Court to reject the claim that the state's RFRA prevents the application of the state's AD law to a photographer declining service to a same-sex couple.[58] Courts in Washington[59] and Hawai'i[60]—states that Lupu

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[56] The analysis in this paragraph assumes the state of the law at the time when the study at the heart of this Article was designed, under which there is no constitutional requirement to exempt religious vendors from generally applicable AD laws. That fact did not change following *Masterpiece Cakeshop,* because the Court found for Phillips on the basis of governmental hostility and did not reach the question of whether Phillips had a right to an exemption from AD laws. 138 S. Ct. 1719, 1723–24, 1732 (2018).

[57] Rhode Island defines "government" to exclude the legislature and the courts and sets the remedies to be "injunctive and declaratory relief against any governmental authority which commits or proposes to commit a violation of this chapter." R.I. GEN. LAWS § 42-80.1 (2010). Connecticut defines "state or any political subdivision of the state" to exclude the legislature and the courts and sets the right to relief only against the state. CONN. GEN. STAT. § 52-571b (1993). New Mexico is very similar to both, as explained below. N.M. STAT. ANN. § 28-22 (2000). Illinois defines "government" to include "a branch" but sets the right to appropriate relief in section 20 only "against a government." 775 ILL. COMP. STAT. 35 (1998).

[58] Elane Photography, L.L.C. v. Willock, 309 P.3d 53, 59, 76 (N.M. 2013) (holding that because the NMRFRA does not apply to the legislator and the courts, and sets remedies only against government agencies, it does not insulate businesses from the legislature's prohibition on discrimination and does not shield them from discrimination lawsuits by private parties, including same-sex couples).

[59] State v. Arlene's Flowers, Inc., 441 P.3d 1203, 1234 (Wash. 2019) (holding that Washington's AD law survives strict scrutiny).

[60] Cervelli v. Aloha Bed & Breakfast, 415 P.3d 919, 934 (Haw. Ct. App. 2018) (holding that, even if the Hawai'i constitution requires strict scrutiny, the Hawai'i AD law survives it).

classifies as hybrid because of RFRA-like constitutional norms[61]—reached a similar result, each ruling that the state's AD law survives strict scrutiny. Overall, a large part of this overlap category appears to be more similar to the AD-only category when it comes to religion-equality conflicts.

The potentially more conflicted overlaps are where RFRAs are construed to apply to state laws (not only executive agencies), without excluding relief against private parties. Such are the Texas and Indiana RFRAs,[62] and new RFRA bills have followed this model.[63] Both the Texas and Indiana RFRAs include language stating that the Act does not authorize or establish a defense for discrimination or breach of civil rights laws for any individual or organization other than religious non-profits.[64] But, as neither state has AD laws that prohibit discrimination on the basis of sexual orientation, these reservations appear to be relevant only in municipalities within these states that enacted local AD protections.[65] These clauses are yet to be interpreted by courts as to whether they resolve the tension or not. More generally, RFRAs do not provide a flat guarantee of exemption, only the possibility of securing an exemption subject to certain legal conditions. Therefore, even expansive RFRA regimes do not guarantee religious vendors a right to refuse to serve same-sex couples, although they increase the likelihood that such

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[61] Lupu, *supra* note 54, at 45–46.

[62] TEX. CIV. PRAC. & REM. CODE ANN § 110.014 (1999) ("A person whose free exercise of religion has been substantially burdened . . . may assert that violation . . . without regard to whether the proceeding is brought in the name of the state or by any other person."); IND. CODE § 34-13-9-7 (2015) ("regardless of whether the state or any other governmental entity is a party to the proceeding").

[63] In addition to the newly enacted Indiana and Mississippi RFRAs, MISS. CODE § 11-61-1 (2014), many recent RFRA bills followed the same structure, including SB 898 in Oklahoma, HB 55 in New Mexico, SB 180 in Kentucky, SB 1062 in Arizona, etc.

[64] TEX. CIV. PRAC. & REM. CODE ANN § 110.014 (1999); IND. CODE § 34-13-9-7 (2015).

[65] It seems that this is also how these RFRA provisions have been understood in the public media. David S. Cohen & Leonore Carpenter, *The "Fix" to Indiana's Law Still Doesn't Protect Hoosiers From Anti-Gay Discrimination*, SLATE (Apr. 2, 2015), https://slate.com/human-interest/2015/04/indiana-religious-freedom-law-the-fix-still-doesnt-protect-gay-hoosiers-from-discrimination.html, *archived at* https://perma.cc/2LSV-G5ZX (arguing that a suggested fix in Indiana's RFRA is relevant only to the few cities that passed AD bills); Robbie Owens, *Texas Has Its Own Religious Freedom Law*, CBS DFW (Mar. 31, 2015), https://dfw.cbslocal.com/2015/03/31/fifteen-year-old-texas-law-similar-to-new-indiana-law/, *archived at* https://perma.cc/6FAK-U52N (claiming the Texas RFRA "can't be misused to disregard civil rights protections.").

right is granted.

In summary, the contemporary regulation of the tension between sexual orientation equality and religious liberty encompasses four legal categories: (1) regimes (state or local) with both AD laws and RFRAs; (2) regimes that only have AD laws; (3) regimes that only have RFRAs; and (4) regimes that have none. This patchwork is the background against which *Masterpiece Cakeshop* was decided, and against which the debate on religious exemptions is raging.

D. Opposing Arguments About the Consequences of Religious Exemptions

The legislative mismatch and the inconsistent patchwork of protections for same-sex couples and religious objectors across the nation yielded two forceful and opposite responses to religious exemption laws.

In one camp are advocates and scholars that emphatically object to the legislation of new RFRAs and to most types of religious exemptions from AD laws. Much of the concern voiced by this group is about harm and consequences, perhaps most strongly articulated in Mark Stern's argument that if there is any religious accommodation, "inevitably, it will soon stretch to restaurants, hotels, movie theaters—in short, to all facets of public life. A religious right to discriminate against gay people will lead directly to anti-gay segregation."[66] Professors Douglas NeJaime and Reva Siegel take the view that claims for religious exemptions reflect the same effort to preserve traditional gender norms that characterized the religious objection to enacting these laws in the first place, what they call "preservation through transformation."[67] Hence, they argue that religious accommodations "may continue democratic conflict in new forms,"[68] and faith claims would escalate in number and significance.[69] Law professors also expressed these concerns

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[66] Mark Joseph Stern, *Anti-Gay Segregation May Soon Be Coming to Oregon*, SLATE (Feb. 4, 2014), https://slate.com/human-interest/2014/02/oregon-anti-gay-referendum-the-initiative-is-homophobic-segregation html, *archived at* https://perma.cc/SYG8-6PUD.

[67] Douglas NeJaime & Reva B. Siegel, *Conscience Wars: Complicity-Based Conscience Claims in Religion and Politics*, 124 YALE L.J. 2516, 2552–54 (2015).

[68] *Id.* at 2521.

[69] *Id*. at 2520; *see also* Douglas NeJaime & Reva Siegel, *Conscience Wars in Transnational Perspective: Religious Liberty, Third-Party Harm, and Pluralism*, *in* THE CONSCIENCE WARS: RETHINKING THE BALANCE BETWEEN RELIGION, IDENTITY, AND

to legislatures deliberating new RFRAs, urging them to reconsider the bills.[70]

In the opposing camp are advocates and scholars—including some supporters of same-sex marriage[71]—who support religious exemptions. This group, which has also been active in communicating with legislators and pushing forward draft proposals for religious exemptions,[72] rejects the consequential concerns as detached from reality. Professor Andrew Koppelman cites data from polls indicating that a majority of Americans and the vast majority of young Americans now support same-sex marriages.[73] Reflecting on the volume of court cases, he then claims that instances of individuals invoking religious exemptions from antidiscrimination laws are extremely rare, "a handful in a country of 300 million people."[74] The economic purposes of antidiscrimination law, Koppelman writes, "are a response to pervasive discrimination, and therefore "they are not frustrated by discrimination which is unusual." Based on the assumption that discrimination against same-sex couples is unusual, he argues that "[i]f gay people are generally protected against discrimination, then a few outliers

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EQUALITY 187 (Susanna Mancini & Michel Rosenfeld eds., 2018).

[70] Letter from Katherine Franke, Isidor & Seville Sulzbacher Prof. of Law, Columbia University, et al., to Ed DeLaney, Rep. of Indiana (Feb. 27, 2015), https://web.law.columbia.edu/sites/default/files/microsites/gender-sexuality/law_professors_letter_on_indiana_rfra.pdf, *archived at* https://perma.cc/P5AN-YMX4 (criticizing the original Indiana RFRA); letter from Ira C. Lupu, F. Elwood & Eleanor Davis Prof. of Law Emeritus, George Washington University, et al., to Gov. Nathan Deal (Jan. 21, 2015), https://georgiaunites.org/wp-content/uploads/2015/01/Georgia-Religious-Freedom-Letter.pdf, *archived at* https://perma.cc/2HB4-LEMP (criticizing the Georgia RFRA proposal).

[71] *See, e.g.*, Andrew Koppelman, *Gay Rights, Religious Accommodations, and the Purposes of Antidiscrimination Law*, 88 S. CAL. L. REV. 619, 620, 643–44 (2014); Douglas Laycock, *Religious Liberty and the Culture Wars*, 2014 U. ILL. L. REV. 839, 877–80 (2014).

[72] For a collection of letters to state legislators making these and similar proposals, see Thomas Berg, *ARCHIVE: Memos/Letters on Religious Liberty and Same-Sex Marriage*, MIRROR OF JUSTICE (Aug. 2, 2009), https://mirrorofjustice.blogs.com/mirrorofjustice/2009/08/memosletters-on-religious-liberty-and-samesex-marriage.html, *archived at* https://perma.cc/A6MN-M2SW. For the model exemption law advanced by this group, see Letter from Edward McGlynn Gaffney, Jr., Prof. of Law, Valparaiso Univ. Sch. of Law, et al., to Rosalyn H. Baker, State Sen., Haw. (Oct. 17, 2013), https://mirrorofjustice.blogs.com/files/hawaii-special-session-letter-10-17-13-1.pdf, *archived at* https://perma.cc/FUK6-H6KS.

[73] Koppelman, *supra* note 71, at 624.

[74] *Id.* at 643.

won't make any difference."[75] Similarly, Professors Thomas Berg and Douglas Laycock argue that states do not have a compelling interest in enforcing their antidiscrimination laws against religious objectors where "ample alternative providers exist (as they nearly always do)."[76] *Masterpiece Cakeshop*, in their view, is precisely one such case because other bakers were readily available to provide the service.[77] Yet, the premise that exemptions should be allowed where market alternatives exist is under-developed in these arguments. How many other bakers would need to be available to justify an exemption? And if a large number of bakers ultimately refused service, would it invalidate an otherwise justified exemption?

The question of what quantity of refusing vendors begins to erode the position of proponents of religious exemptions is left unanswered. Koppelman concedes that, in some areas of the country, many businesses might invoke an exemption; but he immediately dismisses this concern, assuming that these areas do not have antidiscrimination protections in the first place.[78] With respect to *Masterpiece Cakeshop,* Berg and Laycock simply note that the couple accepted an offer of a free wedding cake after being refused by Phillips.[79] They do not consider other potential scenarios—for example that a couple would encounter repeated refusals until finally securing a cake—or considerations—for example, that the risk of refusal might be multiplied by the number of vendors a couple typically contracts with for their wedding. Finally, proponents of religious exemptions do not consider the question of how religious exemptions might *themselves* shape market alternatives. If religious exemptions encourage more refusals, or expand to other facets of public life, as Seigel, NeJaime, and others worry,

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[75] *Id.* at 627.

[76] Douglas Laycock & Thomas C. Berg, *Here's what you missed in the Supreme Court ruling in same-sex wedding cake case*, DALL. NEWS (June 14, 2018), https://www.dallasnews.com/opinion/commentary/2018/06/14/missed-supreme-court-ruling-sex-wedding-cake-case, *archived as* https://perma.cc/QK9T-FYPH.

[77] Thomas C. Berg & Douglas Laycock, Masterpiece Cakeshop *and Reading* Smith *Carefully: A Reply to Jim Oleske*, TAKE CARE (Oct. 30, 2017), https://takecareblog.com/blog/masterpiece-cakeshop-and-reading-smith-carefully-a-reply-to-jim-oleske, *archived at* https://perma.cc/59PP-DKYT ("The case would be different . . . if no other baker were readily available.").

[78] Koppelman, *supra* note 71, at 644.

[79] Brief for Christian Legal Society et al. as Amici Curiae Supporting Petitioners at 30, Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm'n, 138 S. Ct. 1719 (2018) (No. 16-111) [hereinafter Berg & Laycock's Brief].

then the premise of market alternatives could erode further.[80]

It is possible that the proponents of exemptions are not worried about the potential expansion of faith-based claims because they assume that no religious objector would shy away from expressing their objection under current legal prohibitions, and thus, the only live question is how the authorities choose to treat these inevitable objections. This type of thinking is implicit in Berg and Laycock's description of religious objectors:

> Those bakers willing to turn away good business for religious reasons believe that they are being asked to defy God's will, disrupting the most important relationship in their lives, a relationship with an omnipotent being who controls their fates. They believe that they are being asked to do serious wrong that will torment their conscience for a long time after. Petitioner said he would be "dishonoring" and "displeasing" "the sovereign God of the universe."[81]

Berg and Laycock further write that "[t]he harm of regulation on the religious side is permanent loss of identity or permanent loss of occupation."[82] But is the assumption, that religious objection (where there is one) is an inevitable and fixed position, necessarily true? Or might different legal arrangements influence believers to either tolerate or object to same-sex marriage? This, again, is an open empirical question. If religious objection fluctuates in response to the availability of religious exemptions, and individuals who were willing to provide services to same-sex weddings become unwilling to do so once an exemption is announced, it is unclear that the vigor of Berg and Laycock's argument regarding the harm to religious objectors remains intact. In such case, more nuanced questions would need to be explored: What, really, is the magnitude of harm from not being able to refuse service to same-sex weddings? To what extent is refusal the only available religious response? And is it justified to exempt objectors for whom serving same-sex couples would truly disrupt the most important relationship

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[80] NeJaime & Siegel, *supra* note 67, at 2566–74. Koppelman is aware of this concern, but he dismisses such a "cascade" as unlikely given what he considers to be the irreversible trend in social attitudes towards gay couples. Koppelman, *supra* note 71, at 644.

[81] Berg & Laycock's Brief, *supra* note 79, at 31.

[82] *Id*. at 32.

in their lives, if such exemption also causes many other vendors to refuse service that they would have otherwise provided willingly?[83]

II. THE *MASTERPIECE CAKESHOP* EXPERIMENT

A. The Motivation and Setting for the Experiment

The primary purpose of the present experiment was to examine the contradicting empirical assumptions regarding the effects of religious exemptions on discrimination towards same-sex couples. These assumptions lie at the heart of the debate on religious exemptions, particularly in the context of weddings, yet neither side has actual data on the consequences of religious exemptions in this market or elsewhere. Even data on the more basic question—the scope of discrimination towards same-sex couples in the wedding industry or any business market—is lacking. These omissions have made it impossible to assess the merits of the opposing positions and have left the debate hanging in the air.

Masterpiece Cakeshop created an opportunity to evaluate these arguments in their most pressing setting. Based on the oral arguments, I anticipated that the Court would grant an exemption, in one format or another.[84] As "one of the most anticipated decisions of the term,"[85] the decision was also likely to draw extensive coverage and discussion in the public media (as it did), and thus potentially have an impact on public

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[83] This is not an exhaustive list of intriguing empirical questions. One question that I do not address in this Article is that of religious same-sex couples, and how harm to religious interests should be weighed when religion is on both sides of the conflict—the vendor *and* the couple. This could be addressed in future articles.

[84] This expectation was formed based on the comments of Justice Anthony Kennedy, the Court's swing seat, who hinted that the Court thought that there was "a significant aspect of hostility to a religion in this case." Transcript of Oral Argument, *supra* note 12, at 54. This became a dominant line of questioning from the conservative judges on the bench. *Id.* at 54–59. Justice Kennedy also said unequivocally, "Counselor, tolerance is essential in a free society. . . . It seems to me that the state in its position here has been neither tolerant nor respectful of Mr. Phillips' religious beliefs." *Id.* at 64.

[85] Amy Howe, *Opinion Analysis: Court Rules (Narrowly) for Baker in Same-Sex-Wedding-Cake Case [Updated],* SCOTUSBLOG (June 4, 2018), https://www.scotusblog.com/2018/06/opinion-analysis-court-rules-narrowly-for-baker-in-same-sex-wedding-cake-case/, *archived at* https://perma.cc/S7LZ-BVZ2.

attitudes and conduct.[86]

When the decision was finally rendered on June 4, 2018, it received broad coverage and mixed responses. National, state, and local news outlets covered the decision and sought comment from local advocacy groups and politicians.[87] All mainstream outlets, including the New York Times, NBC News, and CNN, titled the decision a victory for the baker; they also called the decision "narrow," explaining that it did not resolve the big constitutional questions at issue.[88] At the same time, many conservative leaders and religious liberty advocates hailed the decision as a victory, expressing significantly less reservations about its scope.[89] Fox News held a supportive

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[86] Katerina Linos & Kimberly Twist, *The Supreme Court, the Media, and Public Opinion: Comparing Experimental and Observational Methods*, 45 J. LEGAL STUD. 223, 247 (2016).

[87] *See, e.g.*, Lauren McGaughy, *Supreme Court Sides With Baker Who Refused To Make Wedding Cake For Gay Couple*, DALL. NEWS (June 4, 2018), https://www.dallasnews.com/news/lgbt/2018/06/04/supreme-court-sides-baker-refused-make-wedding-cake-gay-couple, *archived at* https://perma.cc/T68K-5XXG; Emma Platoff, *What the U.S. Supreme Court's Masterpiece Cakeshop decision means for religious refusal laws in Texas*, TEX. TRIB. (June 5, 2018), https://www.texastribune.org/2018/06/05/us-supreme-court-masterpiece-cakeshop-gay-ruling-religious-freedom-tex/, *archived at* https://perma.cc/BW9D-46TF; Katie Simpson, *New Supreme Court Ruling May Affect Indiana Religious Freedom Lawsuit*, WFYI INDIANAPOLIS (June 4, 2018), https://www.wfyi.org/news/articles/new-supreme-court-ruling-may-affect-indiana-religious-freedom-lawsuit, *archived at* https://perma.cc/D6YY-DTR5 (describing *Masterpiece* as a victory for religious exemptions which may assist conservative groups to challenge Indiana's "weakening religious freedom protections").

[88] Mark Goldfeder, *How the Supreme Court (respectfully) kicked the cake down the road*, CNN (June 6, 2018), https://edition.cnn.com/2018/06/04/opinions/supreme-court-masterpiece-cakeshop-goldfeder/index html, *archived at* https://perma.cc/HAG8-UEGC ("Initial reviews . . . mostly imply that it was a very narrow ruling and is therefore somewhat unremarkable."); Adam Liptak, *In Narrow Decision the Supreme Court Sides with Baker Who Turned Away Gay Couple*, N.Y. TIMES (June 4, 2018), https://www nytimes.com/2018/06/04/us/politics/supreme-court-sides-with-baker-who-turned-away-gay-couple.html, *archived at* https://perma.cc/655G-72YQ ("The court's decision was narrow . . . . The court passed on an opportunity to either bolster the right to same-sex marriage or explain how far the government can go in regulating businesses run on religious principles."); Pete Williams, *In narrow ruling, Supreme Court gives victory to Baker Who Refused To Make Cake For Gay Wedding*, NBC NEWS (June 4, 2018), https://www nbcnews.com/politics/supreme-court/narrow-ruling-supreme-court-gives-victory-baker-who-refused-make-n872946, *archived at* https://perma.cc/KXG3-74PY ("[T]he opinion was a narrow one, applying to the specific facts of this case only.").

[89] Emilie Kao, *Why the Supreme Court's Ruling for a Christian Baker Was Not 'Narrow'*, DAILY SIGNAL (June 12, 2018), https://www.dailysignal.com/2018/06/12/why-the-supreme-courts-ruling-for-a-christian-baker-was-not-narrow/, *archived at* https://perma.cc/ECS6-7D72 ("the decision . . . expos[ed] a huge fallacy in the ACLU's

interview with Phillips, who defined the decision as a "big win."[90] Leaders of the U.S. Conference of Catholic Bishops released a joint statement applauding the decision, saying that it "confirms that people of faith should not suffer discrimination on account of their deeply held religious beliefs, but instead should be respected by government officials," emphasizing the decision's expression of pluralism and tolerance.[91] The Family Research Council released a statement that the decision "made clear that the government has no authority to discriminate against Jack Phillips because of his religious beliefs" and that the "ruling means Jack will remain free to live according to his beliefs whether he is at work, at home, or in his place of worship."[92] These statements do not betray any doubt about the scope of the decision or mention its recognition of the important role of AD laws in

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main argument in the case . . . The court's clear rejection of the discrimination argument has implications for many of the other conflicts currently brewing between religious freedom and sexual orientation."); *Victory for Colorado Cake Case*, LIBERTY COUNS. (June 4, 2018), https://www.lc.org/newsroom/details/060418-victory-for-colorado-cake-case, *archived at* https://perma.cc/9M8L-QZ23 ("Though the Court focused on the explicit hostility exhibited by the Colorado Civil Rights Commission in this specific instance, this significant decision will have a wide impact regarding the clash between free speech and the LGBT agenda, including laws that add 'sexual orientation' and 'gender identity.'").

[90] *Colorado Baker Reacts to 'Big Win' in Same-Sex Wedding Cake Case*, FOX NEWS INSIDER (June 5, 2018), https://insider foxnews.com/2018/06/05/same-sex-wedding-cake-case-colorado-baker-jack-phillips-supreme-court-ruling-was-big-win, *archived at* https://perma.cc/3Z2C-PDRP; *see also* Todd Starnes, *A win for Masterpiece Cakeshop but it ain't over yet*, FOX NEWS (June 4, 2018), https://www.foxnews.com/opinion/todd-starnes-a-win-for-masterpiece-cakeshop-but-it-aint-over-yet, *archived at* https://perma.cc/8STY-5Q5Z ("Monday's ruling should give some comfort to Christian business owners who primarily service the wedding industry – gay rights do not necessarily trump everyone else's rights."). Other coverage by Fox News was more careful in discussing the limitations of the decision. *See, e.g.*, Bill Mears & Judson Berger, *Supreme Court sides with Colorado baker who refused to make wedding cake for same-sex couple*, FOX NEWS LIVE (June 4, 2018), https://www.foxnews.com/politics/supreme-court-sides-with-colorado-baker-who-refused-to-make-wedding-cake-for-same-sex-couple, *archived at* https://perma.cc/6YHF-XMS9 ("The narrow ruling here focused on what the court described as anti-religious bias on the Colorado Civil Rights Commission when it ruled against baker Jack Phillips.").

[91] *Religious freedom groups praise Supreme Court's Masterpiece ruling*, CATH. NEWS AGENCY (June 4, 2018), https://www.catholicnewsagency.com/news/religious-freedom-groups-praise-supreme-courts-masterpiece-ruling-57089, *archived at* https://perma.cc/NV9W-38UR.

[92] *Supreme Court Ruling a Victory for Freedom of Colorado Baker to Live by his Faith, says Family Research Council*, FAM. RSCH. COUNCIL (June 4, 2018), https://www frc.org/newsroom/supreme-court-ruling-a-victory-for-freedom-of-colorado-baker-to-live-by-his-faith-says-family-research-council, *archived at* https://perma.cc/4Q7L-Q5FX.

protecting against sexual orientation discrimination.

Some progressive commentators observed these enthusiastic responses and voiced concerns that *Masterpiece Cakeshop* will grant objectors a license to discriminate. Gay and Lesbian Alliance Against Defamation ("GLAAD") President and CEO, Sarah Kate Ellis, said that it "leaves the door wide open for religious exemptions to be used against LGBTQ people."[93] Annise Parker, the President of the LGBTQ Victory Institute, further warned that, "[h]omophobic forces will purposefully over-interpret the ruling and challenge existing non-discrimination laws by refusing service to LGBTQ people in even more situations."[94] NBC News columnist, Scott Lemieux, wrote that the decision "presents a serious risk of undermining civil rights law in the name of religious freedom, especially given that it invites yet further suits for the court to consider."[95]

This combination of factors—a highly anticipated decision, a court that appeared positioned to exempt the religious objector, and the massive coverage that followed the decision and communicated the above messages—created a favorable setting for the empirical test of the effects (or lack thereof) of religious exemptions on sexual orientation discrimination. In a previous study, Professors Katerina Linos and Kimberly Twist found that Supreme Court decisions can increase support for controversial policies that were vindicated by the Court (e.g., the Affordable Care Act), even when the court was divided and the decision was nuanced.[96] Similarly, three recent studies, measuring the effect of the legalization of same-sex marriage on public attitudes, documented an increase in perceptions that social norms support same-sex marriage[97] and an increase in personal support for same-

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[93] Nico Lang, *Hate Groups Want to Exploit Masterpiece Cakeshop Ruling as a License to Discriminate*, INTO (June 4, 2018), https://www.intomore.com/impact/hate-groups-want-to-exploit-masterpiece-cakeshop-ruling-as-a-license-to-discriminate/, *archived at* https://perma.cc/7DLD-67AR.

[94] *Id.*

[95] Scott Lemieux, *How the 'Narrow' Ruling in Masterpiece Cakeshop Could Undermine Future Civil Rights Cases*, NBC NEWS (June 5, 2018), https://www nbcnews.com/think/opinion/how-narrow-ruling-masterpiece-cakeshop-could-undermine-future-civil-rights-ncna879976, *archived at* https://perma.cc/YM2M-9EZ6.

[96] Linos & Twist, *supra* note 86, at 247.

[97] Margaret E. Tankard & Elizabeth Levy Paluck, *The Effect of a Supreme Court Decision Regarding Gay Marriage on Social Norms and Personal Attitudes*, 28 PSYCH. SCI. 1334, 1339 (2017).

sex marriages[98] post-*Obergefell*, as well as a sharper decrease in antigay bias in states that legalized same-sex marriage compared with those that did not.[99] All of these studies were based on attitudinal surveys conducted shortly before and after the decisions or acts of legislation, sometimes with an additional experimental component that randomized the framing of the decision or the information provided on the decision. Yet none of these studies examined the implications of Supreme Court decisions on the behavior of decision-makers pertinent to the subject matter of the decision (in the present case, how wedding vendors are influenced from a decision pertinent to the wedding industry).

In addition, previous studies did not investigate whether effects of Supreme Court decisions vary between socio-legal regimes. As Part II explained, the variation in how states regulate sexual orientation discrimination and religious freedom is potentially important in the present case, as these background regimes yield different expectations about the legal outcomes of otherwise identical cases. These expectations could have directed wedding vendors towards different behaviors and could have differentiated their response to the *Masterpiece Cakeshop* decision. For example, business in regimes that resemble Colorado—with AD laws and without RFRAs—might refuse service to same-sex couples to a greater extent post-*Masterpiece Cakeshop* if they believe that *Masterpiece Cakeshop* relaxed their AD obligations. One may also expect this change to be more pronounced in overlap regimes, because the existence of a RFRA could strengthen the impression that businesses are likely to secure an exemption post-*Masterpiece Cakeshop*. In contrast, businesses in regimes that have never enacted AD laws have no legal basis to change their behavior. For these businesses, the law has not changed: they are as free to discriminate after *Masterpiece Cakeshop* as they were before the ruling. All these hypotheses should be couched in the general caveat that businesses are not necessarily well versed in the law. Therefore, it is also possible that businesses in different legal regimes would not respond differently to *Masterpiece*

[98] Emily Kazyak & Mathew Stange, *Backlash or a Positive Response?: Public Opinion of LGB Issues After* Obergefell v. Hodges, 65 J. HOMOSEXUALITY 2028, 2040 (2018).

[99] Eugene K. Ofosu et al., *Same-Sex Marriage Legalization Associated With Reduced Implicit And Explicit Antigay Bias*, 116 PROC. NAT'L ACAD. SCI. U.S. 8846, 8849–51 (2019).

Cakeshop. But then again, laws are not enacted at random, but are the product of certain social and political conditions. These conditions could in turn influence the acceptance and interpretation of the decision, even if businesses are not directly aware of their rights and obligations under the law. In short, investigating differences between socio-legal regimes is vital to understand whether the effect of a national Supreme Court decision is general or varies from one regime to another.

In sum, *Masterpiece Cakeshop* provided a unique opportunity to study the behavioral effect of providing a religious exemption from antidiscrimination laws, a question of which no empirical data exist to date, and which bears heavily on contemporary legal debates. In addition, the present study goes deeper than previous studies in probing the relationship between the national Supreme Court "shock" and the preexisting sub-national legal structures that could vary the effect of the decision between otherwise similar regimes.

B. Research Design

To assess whether *Masterpiece Cakeshop* had an effect on sexual orientation discrimination in the wedding industry, I combined methods from natural (pseudo) experiments and field experiments. As in such experiments, I examined the behavior of wedding businesses in two periods: before (May 8–15, 2018) and after (June 13–20, 2018) the decision (June 4, 2018). As in field experiments, the methods aimed to control for both the setting of the examination and the allocation of sexual orientation treatment between businesses, to allow for causal inference.

Sample construction began with a preliminary comparison of all states, to find those that were most comparable in their overall characteristics yet differed in legal regime. Four states were selected: Indiana, Texas, Iowa, and North Carolina. Table 1 shows that these states have roughly the same attitudinal and economic characteristics yet vary in how they regulate religious freedom and public accommodations. North Carolina has no RFRA and no AD law at any level of government (-RFRA, -AD). Iowa has no RFRA (at any level of government), but has a state AD law (-RFRA, +AD).[100]

[100] IOWA CODE § 216.7 (2017). Notably, the Iowa Supreme Court has been a

Indiana and Texas model together the two final categories: both have state RFRAs[101] and no state AD laws, yet some jurisdictions within these states have local AD laws.[102] Texas and Indiana jurisdictions with AD laws model the +RFRA, +AD category, whereas Texas and Indiana jurisdictions without such laws model the +RFRA, -AD category.

TABLE 1 – CHARACTERISTICS OF SAMPLED REGIMES

Criterion	Definition	IA[103]	NC[104]	IN[105]	TX[106]	Dallas	Houston

trailblazer for gay rights, striking down Iowa's anti-sodomy law twenty-seven years before the U.S. Supreme Court did the same in *Lawrence v. Texas*, 539 U.S. 558, 578–79 (2003). State v. Pilcher, 242 N.W.2d 348, 359–60 (Iowa 1976) (en banc) (holding Iowa's criminal anti-sodomy law unconstitutional under the Fourteenth Amendment as applied to "adult persons of the opposite sex"). Iowa also became the third state in the nation to allow same-sex couples to marry when the Iowa Supreme Court legalized same-sex marriage in *Varnum v. Brien*, 763 N.W.2d 862, 907 (Iowa 2009). *See also Iowa Supreme Court legalizes gay marriage*, NBC NEWS (Apr. 3, 2009), https://www nbcnews.com/id/wbna30027685, *archived at* https://perma.cc/Q6BH-2LUD. RFRA has been repeatedly proposed and rejected in the state legislature. Barbara Rodriguez, *Controversial 'religious freedom' bill gets another look at Iowa Capitol*, DES MOINES REG. (Feb. 18, 2019), https://www.desmoinesregister.com/story/news/politics/2019/02/18/iowa-republicans-religious-freedom-restoration-act-capitol-rfra-discrimination-bill/2909230002/, *archived at* https://perma.cc/CT95-6RPZ.

[101] IND. CODE § 34-13-9 (2015); TEX. CIV. PRAC. & REM. CODE ANN § 110.011 (1999).

[102] Jurisdictions in Indiana with AD laws include Indianapolis, Fort Wayne, Evansville, Bloomington, Muncie, South Bend, and Terre Haute. *Indiana's Equality Profile*, *supra* note 55. Jurisdictions in Indiana without AD laws include West Lafayette. *Id.* Jurisdictions in Texas with AD laws include Dallas, San Antonio, Austin, El Paso, Plano, and Fort Worth. *Texas' Equality Profile*, *supra* note 55. Jurisdictions in Texas without AD laws include Houston, Irving, Arlington, Corpus Christi, Lubbock, Garland, Amarillo, Grand Prairie, Brownsville, McKinney, Killeen, McAllen, Waco, Denton, Round Rock, and College Station. *Id.*

[103] PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN IOWA (2015), https://www.pewforum.org/religious-landscape-study/state/iowa/, *archived at* https://perma.cc/2X9G-YX76.

[104] PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN NORTH CAROLINA (2015), https://www.pewforum.org/religious-landscape-study/state/north-carolina/, *archived at* https://perma.cc/3NGA-EPKU.

[105] PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN INDIANA (2015), https://www.pewforum.org/religious-landscape-study/state/indiana/, *archived at* https://perma.cc/TGF4-5LQ5.

[106] PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN TEXAS (2015), https://www.pewforum.org/religious-landscape-study/state/texas/, *archived at*

						Metro, TX[107]	Metro, TX[108]
GDP ($)[109]		59,978	54,442	55,173	61,168	--	--
Importance of Religion	Religion is Somewhat/Very Important (National average: 77%)	79%	84%	78%	86%	85%	83%
% Conservatives	(National average: 36%)	41%	40%	41%	39%	41%	38%
% Evangelicals	(National average: 25%)	28%	35%	31%	31%	38%	30%
Attitudes Towards Homosexuals	"Homosexuality should be discouraged" (National average: 31%)	36%	36%	37%	36%	35%	39%
Attitudes Towards Same-Sex Marriage	Opposing/Strongly Opposing Same-Sex Marriage (National average: 39%)	41%	45%	45%	46%	44%	51%
State RFRA[110]		No	No	Yes	Yes	Yes	Yes
State/Local AD law[111]		Yes	No	Some	Some	Yes	No

Two reasons were responsible for the choice of Texas and Indiana as models of the overlap category (+RFRA, +AD) and the +RFRA, -AD category. As Part II describes, there are three versions of the overlap between RFRAs and AD laws: (1) states that enacted both laws; (2) states that enacted an AD law and whose courts interpret the constitution to provide a RFRA-like standard; and (3) local AD laws within RFRA states. The primary reason

https://perma.cc/DNF2-4PPF.

[107] PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN THE DALLAS METRO AREA (2015), https://www.pewforum.org/religious-landscape-study/metro-area/dallasfort-worth-metro-area/, *archived at* https://perma.cc/9XQ4-79JH.

[108] PEW RSCH. CTR., RELIGIOUS LANDSCAPE STUDY: ADULTS IN THE HOUSTON METRO AREA (2015), https://www.pewforum.org/religious-landscape-study/metro-area/houston-metro-area/, *archived at* https://perma.cc/5F7L-Y3ZS.

[109] GDP per capita was calculated based on data from the second quarter of 2018. BUREAU OF ECON. ANALYSIS, U.S. DEP'T OF COM., GROSS DOMESTIC PRODUCT BY STATE: SECOND QUARTER 2018 (2018).

[110] IND. CODE § 34-13-9 (2015); TEX. CIV. PRAC. & REM. CODE ANN § 110.011 (1999).

[111] This refers to public accommodation laws that apply to private businesses and are enacted at the state or city level.

for choosing the third version to model the overlap category was that the demographic and attitudinal characteristics of the four states that enacted both laws (RI, CN, NM, IL) and the states that had a RFRA without an AD law differed widely from states in the three other categories. Second, as Part II discusses, the particular RFRA design in the first overlap category was not conducive for the examination of the tension between RFRA and AD laws, while the second overlap category raised considerable uncertainty regarding the existence of the same tension. Texas and Indiana provided an adequate demographic and attitudinal comparison to the other legal categories, as well as clarity regarding the classification of their legal regimes.

To be sure, I do not argue that the design is capable of identifying a *causal relationship* between specific regimes and behavioral outcomes (as I will show next, other features of the design allow for the identification of a causal relationship in the entire sample, *across* legal regimes). First, background laws—unlike the experimental treatment—are not randomly assigned. They cannot be easily separated from the underlying political and social climate that produced them.[112] In addition, unlike the *Masterpiece Cakeshop* decision, they are not new, so their effect cannot be studied as a pseudo, natural experiment. Second, as discussed above, while different laws provide different behavioral guidance, businesses may not be fully aware of laws' dictates. Nevertheless, it is important to study the variation between legal regimes—if not for the direct impact of law, then for the potential impact of the underlying socio-political structures that the law reflects. Had I only sampled from one regime, important real-world variation would have been masked. Exploring how businesses in different regimes respond to *Masterpiece Cakeshop* is necessary, even if the results are only suggestive and causal inference is limited.

The sample was built by collecting information on photographers, bakers, and florists in each legal regime through a Google search, aiming to include 250 vendors per regime.[113] Only vendors who published an email address

[112] In addition, the law is determined not only based on acts of the legislature but also based on judicial decisions and administrative directives that interpret the enacted rule. I attempted to account for those—for example, by not sampling from overlap states where courts interpreted RFRAs as providing no protection against AD claims—but it is very difficult to account for all interactions between judge-made law and legislated law.

[113] For an extended discussion of the sample, see Barak-Corren, *supra* note 18, at 12–

were sampled.[114] Vendors were contacted by email, a highly common method for communication in the wedding market. There is ample guidance online on how to write an email to potential vendors, and multiple websites assume that email is the default or best form of communication with vendors.[115]

Next, sixteen fictitious email profiles were created to facilitate the experiment. In order to assess the baseline discrimination pattern, each business received two emails prior to *Masterpiece Cakeshop* from two different "couples": a same-sex couple (first wave) and a different-sex couple (second wave). The couples' sexual orientation was made evident by their names. The name of the sender, appearing in the profile information and the signature, was a generic American male name (John, Robert, Dylan, Scott). The name of the prospective spouse appeared inside the body of the email and was a generic name for an American male or female, depending on the couple's identity (Adam, Paul, Harry; Ashley, Rebecca, Jessica).[116] The

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17. The choice of vendors was influenced by recent cases in which businesses refused service to same-sex couples. *See, e.g.*, State v. Arlene's Flowers, Inc., 441 P.3d 1203 (Wash. 2019) (florists); Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm'n, 138 S. Ct. 1719 (2018) (bakers); Elane Photography, L.L.C. v. Willock, 309 P.3d 53 (N.M. 2013) (photographers).

[114] Vendors that did not publish an email address typically had an online application form on their website, reducing the potential concern that the sample is biased towards technology-oriented vendors.

[115] *See, e.g.*, Kelsey Malie, *How to Successfully Communicate With Your Wedding Vendors*, (Mar. 29, 2018), http://www kelseymaliecalligraphy.com/blog/2018/3/29/how-to-successfully-communicate-with-your-wedding-vendors, *archived at* https://perma.cc/JD6V-M3KY ("An email is usually the preferred method for inquiries as it allows the vendor to keep track of your conversation, respond in length and from a desktop, and allows them to easily attach files, reference links, and more."); Kim Forrest, *7 Ways to Effectively Communicate With Wedding Vendors*, WEDDINGWIRE (Feb. 13, 2017), https://www.weddingwire.com/wedding-ideas/7-ways-to-effectively-communicate-with-wedding-vendors, *archived at* https://perma.cc/RNK2-U55G (assuming at least some communication is done via email); Adair Currie, *How to Email Potential Wedding Vendors*, EVERY LAST DETAIL (Feb. 3, 2015), https://theeverylastdetail.com/email-potential-wedding-vendors/, *archived at* https://perma.cc/32UF-38KE (providing guidance on how to write emails to potential wedding vendors).

[116] These are highly popular names of men and women respectively (and not of the other gender), for people born in the U.S. in the 1980s and 1990s. *See, e.g.*, SOC. SEC. ADMIN., TOP NAMES OF THE 1990S, https://www.ssa.gov/oact/babynames/decades/names1990s html, *archived at* https://perma.cc/JT2B-XJTW. Those years are the relevant age cohorts for marriage in 2018, when the experiment was conducted. In 2018, the median age for marriage in the U.S. was 30 for men and 28 for women. *See* A.W. Geiger & Gretchen Livingston, *8 Facts About Love and Marriage in America*, PEW RSCH. CTR. (Feb. 13, 2019),

emails had similar properties, including similar information about the fictitious couple and the service requested from the vendor; they were written in the same level of cordiality. Small, meaningless changes were inserted to diminish suspicion (including variations in font size, font color, signature style, and profile pictures).[117] The emails were sent one week apart, about the same time during the week and day, with an intentional hour lag to reduce suspicion.[118]

A week after *Masterpiece Cakeshop*, on June 13, all businesses were randomized to receive an email from a same-sex or a different-sex couple (third wave); and on the following week, each business received an email from the opposite-orientation couple (fourth wave). In each third and fourth wave, the two emails had similar properties and were different from the two pre-*Masterpiece Cakeshop* emails. Each email was always sent from a profile that has not contacted that business before. Altogether, each business received four different emails from four different profiles. Following the same procedure and schedule, a "control" group of businesses were contacted in the third and fourth waves.[119] These businesses were contacted for the first time after the decision, to evaluate the possibility that the repeated measurement of the experimental procedure had an independent effect on business behavior.[120]

C. Strengths and Weaknesses of the Experiment

The experimental design has multiple methodological strengths. First, it combines two of the most powerful methods for causal inference—pseudo-experiments and field experiments—to enable the study of an actual, concrete event—the *Masterpiece Cakeshop* decision—in a controlled setting. Second,

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https://www.pewresearch.org/fact-tank/2019/02/13/8-facts-about-love-and-marriage/, *archived at* https://perma.cc/UL9E-FKVE.

[117] *See* Barak-Corren, *supra* note 18, online app. § OA1, at 1–10, https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf, *archived at* https://perma.cc/GF73-AHXU. All email versions are included in this section of the appendix. *Id.*

[118] Barak-Corren, *supra* note 18, at 18. A small group of subjects received each email 24 or 48 hours after the main group, due to logistical issues. *Id.* at 18 n.22.

[119] *Id.* at 17. There were 251 vendors. *Id.* Additional details on the composition of the control group can be found at this source. *Id.*

[120] *Id.* at 17–20. Further details on the procedure and treatment of the data can be found at this source. *Id.*

sending carefully designed materials of fictitious individuals instead of real auditors creates a controlled setting for the study and removes inadvertent auditor biases.[121] The emails enable controlling the couple's identities, how they represent themselves to businesses, the exact content of the inquiry, and the timing of the inquiry. All of these are very difficult to achieve in studies that employ real testers (audit studies). Although testers can be trained to behave similarly, it is impossible to erase the numerous differences between real people, or control for nuances in tone and facial expressions that can disclose the auditors' attitudes or that their search for a job/service is ingenuine.[122] In addition, while email inquiries do not capture the entire variation in how couples interact with vendors, emails are one of the most common methods of communication between couples and vendors, especially in the inquiry phase.[123] To the extent that the process of negotiating with vendors has even moderate friction, one would expect that reduced positive responses to emails would ultimately translate into less market opportunities for same-sex couples. Third, the outcome measure—agreement to provide services to the couple—is less crude than, for example, callbacks in employment experiments that were used in previous prominent studies.[124] This is because the conflict about discrimination in wedding services focuses on the specific stage of the transaction that is studied here: the initial inquiry about the service.[125]

Alongside these strengths, the experiment also has limitations. First, similar to other studies of discrimination in the field, I study asynchronous communication rather than face to face or phone communication.[126] As noted, there are good reasons for that. However, how the results translate to additional methods of communication remains an open question and a topic for a future study. In addition, the experiment examines willingness to

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[121] Marianne Bertrand & Sendhil Mullainathan, *Are Emily and Greg More Employable Than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination*, 94 AM. ECON. REV. 991, 993–94 (2004).

[122] *Id.*

[123] *See, e.g.*, sources cited *supra* note 115.

[124] Bertrand & Mullainathan, *supra* note 121, at 997.

[125] *See* Masterpiece Cakeshop, Ltd. v. Colo. C.R. Comm'n, 138 S. Ct. 1719, 1724 (2018) (inquiry about purchasing a cake); State v. Arlene's Flowers, Inc., 441 P.3d 1203, 1211 (Wash. 2019) (inquiry about floral arrangements); Elane Photography, L.L.C. v. Willock, 309 P.3d 53, 59–60 (N.M. 2013) (inquiry about wedding photos).

[126] Bertrand & Mullainathan, *supra* note 121, at 991–93.

provide service to male couples and does not examine impacts on lesbian or non-binary couples or couples with distinctively Black or non-white names, nor does it explore the intersectionality of gender and race. This, too, could be a topic for a future study.[127]

An additional limitation, resulting from the pseudo- and controlled experiment design, is that I examine the effect of *Masterpiece Cakeshop* in a relatively short time span: several weeks after the ruling. While collecting more observations would have been desirable, it was not possible to continue isolating the effect of the decision from intervening political developments beyond that period. I explain this in more length in the discussion.

TABLE 4: OVERALL RESPONSE RATES IN EACH WAVE

Wave	Overall Response Rate
W1	70.8
W2	58.7
W3	63.4
W4	61.9

Finally, I encountered a large attrition of businesses in the second wave of inquiries before *Masterpiece Cakeshop* (see Table 4)—an issue that pervades studies that repeatedly measure the same respondents over time.[128]

[127] *See, e.g.*, Kathryn M. Kroeper et al., *Marriage Equality: On the Books and on the Ground? An Experimental Audit Study of Beliefs and Behavior towards Same-Sex and Interracial Couples in the Wedding Industry*, 19 ANALYSES OF SOC. ISSUES & PUB. POL'Y 50 (2019). Kroeper's study was conducted before *Masterpiece*, finding that communications from same-sex couples were ignored more than communications from heterosexual and interracial couples. *Id.* at 66–67. The study did not find meaningful differences between gay and lesbian couples. *See* Kroeper et al.'s online supplement at 3. The comparisons and intersections explored in the study should be revisited, both because the sample size of each couple type was quite small, and because social norms may change, for example, because of decisions such as *Masterpiece Cakeshop*.

[128] *See, e.g.*, Graham Kalton, *Designs for Surveys Over Time*, *in* SAMPLE SURVEYS: INFERENCE AND ANALYSIS 89, 101–03 (C. R. Rao & D. Pfeffermann eds., 2009); ALAN S. GERBER & DONALD P. GREEN, FIELD EXPERIMENTS: DESIGN, ANALYSIS, AND

This pattern hindered the ability to detect discrimination in the pre-*Masterpiece Cakeshop* period, as the first wave of emails was from same-sex couples and the second wave of emails was from opposite-sex couples. While the causes for this attrition are not entirely clear (this is common to studies that encounter attrition),[129] a random phone survey suggested that businesses that provided no response to the second wave of emails were generally less responsive than other businesses (also over the phone), rather than suspicious or email fatigued.[130] To minimize the impact of attrition on the robustness of the design, I randomized couples' identity within each following wave. In addition, the following waves were designed to increase responsiveness by altering the style and formatting of the emails and the couples' profiles. This effort succeeded in increasing responsiveness to wave three and in reducing attrition between waves three and four. Nevertheless, I concede that the attrition of businesses from wave two prevents the evaluation of the existence and extent of sexual orientation discrimination before *Masterpiece Cakeshop*.[131] To overcome this pitfall and evaluate the effect of *Masterpiece Cakeshop* on the existence and extent of discrimination *after* the decision, I developed several strategies of analysis which I present next.

D. Findings

In this section, I present the core results of the *Masterpiece Cakeshop* field experiment.[132] The analysis begins by focusing on businesses that agreed to serve same-sex couples before *Masterpiece Cakeshop* and examining their behavior post-*Masterpiece Cakeshop*. The second analysis examines within-business changes of behavior across all businesses over time. I then move to examining differences between legal jurisdictions and between religious environments.

INTERPRETATION 236–40 (Ann Shin ed., 1st ed. 2012).

[129] John Fitzgerald et al., *An Analysis of Sample Attrition in Panel Data: The Michigan Panel Study of Income Dynamics*, 33 J. HUM. RES. 251, 252 (1998).

[130] *See* Barak-Corren, *supra* note 18, online app. § OA3, at 11–14, https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf, *archived at* https://perma.cc/GF73-AHXU.

[131] *See id.* § OA4.1, at 15–20. It is possible to infer that, prior to *Masterpiece Cakeshop*, opposite-sex couples were disfavored relative to same-sex couples (reverse discrimination), but this inference seems tenuous. To the extent it is true, the magnitude of the *Masterpiece Cakeshop* effect is much larger than estimated below.

[132] For elaborations, robustness checks, and follow up studies, see Barak-Corren, *supra* note 18.

1. Did *Masterpiece Cakeshop* Increase Discrimination Towards Same-Sex Couples?

To answer this question, I evaluate the impact of *Masterpiece Cakeshop* on the 576 businesses that agreed to serve same-sex couples before the decision. Examining their behavior after the decision can provide an answer as to whether *Masterpiece Cakeshop* had a negative effect on the willingness of businesses to provide services to same-sex couples. As all businesses, these previously "gay-friendly" businesses were randomized post-*Masterpiece Cakeshop* to receive an inquiry either from a same-sex or an opposite-sex couple (and then vice versa in the following wave, such that each business was contacted by both couples post-*Masterpiece Cakeshop*). This design allowed me to estimate the effect of *Masterpiece Cakeshop* precisely, using both within and between businesses data.

Overall, post-*Masterpiece Cakeshop* inquiries from a same-sex couple had a 66.3% chance of receiving a positive response. Equivalent inquiries from an opposite-sex couple have a 75.5% chance of being answered positively. This represents a difference of 9.2 percentage points, a 14% change, that can be solely attributed to the identity of the couple;[133] these results were stable and significant in both waves following *Masterpiece Cakeshop*.

How do businesses communicate negative responses to couples? The most common form of declining service is simply no response. This result is anticipated, as writing a negative response is both time-intensive and awkward, and the easiest way for a business to proceed is to ignore the inquiry.[134] While some non-responses may have had other causes—for example, non-receipt of the initial email or simple forgetfulness—we would expect such errors to distribute randomly and therefore equally across couple types. This is not the case. Opposite-sex couples had a 19.6% chance of not receiving a response to their inquiry, while same-sex couples had a 27.8% chance of not receiving a response. That is, the chance of same-sex couples to not receive a response was 42% higher.[135] In addition, while explicitly negative responses were less common, the increase in such responses for same-sex couples from before to after *Masterpiece Cakeshop* was 177% the

[133] $p = .0006$. *Id.* at 24 tbl.5.

[134] *See* John F. Dovidio & Samuel L. Gaertner, *Aversive Racism*, 36 ADVANCES IN EXPERIMENTAL SOC. PSYCH. 1, 10 (2004); *cf.* Bertrand & Mullainathan, *supra* note 121, at 1006.

[135] $Z = 3.26$, $p = .001$. Barak-Corren, *supra* note 18, at 26.

increase of such responses for opposite-sex couples.[136]

The negative effect of *Masterpiece Cakeshop* on the willingness to provide services to same-sex couples was consistent across additional analyses. I find the effect in the entire sample of businesses in the experiment (N=906): comparing the rate of positive responses to same-sex couples before and after *Masterpiece Cakeshop* yields a drop of 14.4 percentage points, or about 23% change. I find the negative effect also in the control group, where the gap between couple types after *Masterpiece Cakeshop* was 9.5 percentage points, or about 14% change. I also find this effect among the particularly keen group of businesses that responded positively to both couples before *Masterpiece Cakeshop*. While these businesses remain more responsive than any other group of businesses, they too differentiate significantly between same-sex and opposite-sex couples after *Masterpiece Cakeshop* (~7 percentage point difference, or about 8% change). The summary of these results is presented in Figure 1, which shows that all business cohorts respond to *Masterpiece Cakeshop* with unfavorable treatment of same-sex couples, notwithstanding different baselines of positive response rates that characterize each cohort separately. In all of these analyses, the *Masterpiece Cakeshop* effect is robust to the inclusion of all experimental covariates, such as the type of business, the legal regime, and so on. The effect is equally strong in urban areas, which are often assumed to be particularly inclusive of same-sex couples, and does not vary with political conservativeness. However, as I report in the next sub-section, the effect varies with religiosity of the business environment, such that businesses in areas dense with religious congregations are more likely to show substantial discrimination towards same-sex couples post-*Masterpiece Cakeshop*.

[136] Z = 2.14, p = .03. Barak-Corren, *supra* note 18, online app. § OA4, at 19, https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf, *archived at* https://perma.cc/GF73-AHXU.

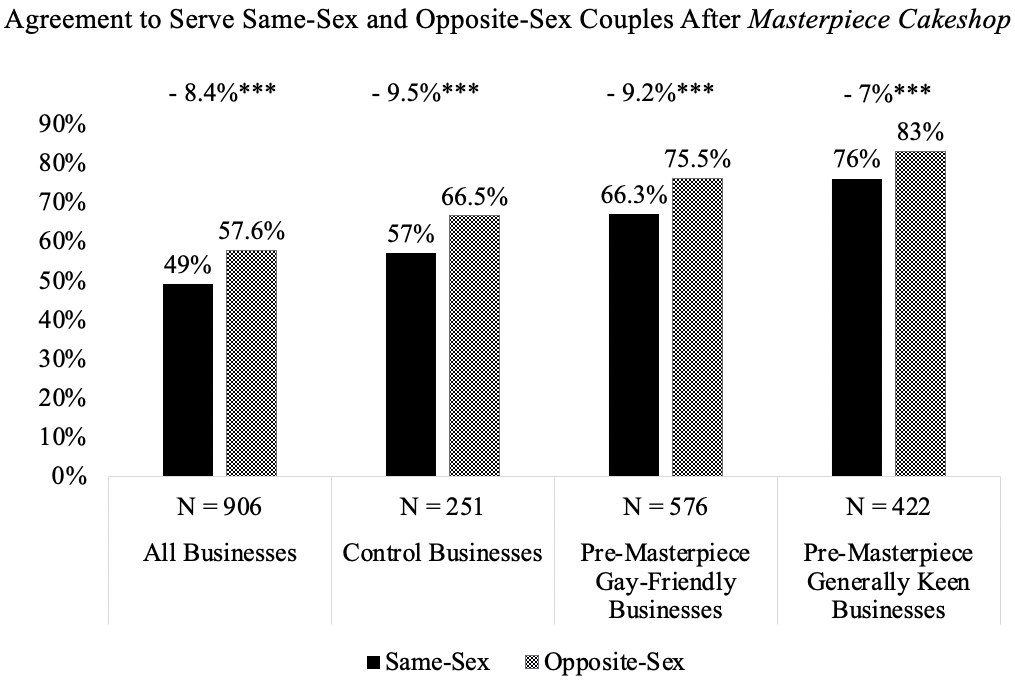

Figure 1.[137] The average positive response rate to same-sex and opposite-sex couples after the *Masterpiece Cakeshop* decision, by four groups of businesses: all businesses; businesses that were sampled for the first time after *Masterpiece Cakeshop* (control businesses); businesses that, prior to *Masterpiece Cakeshop*, were willing to serve same-sex couples (pre-*Masterpiece Cakeshop* gay-friendly businesses); and businesses that, prior to *Masterpiece Cakeshop*, were generally keen to serve all couples (pre-*Masterpiece Cakeshop* generally keen businesses). Gaps in percentage points are noted. *** $p < .01$.

2. What is the Magnitude of the *Masterpiece Cakeshop* Effect?

How substantial are these effects? Take the average 9% gap in willingness to serve same-sex and opposite-sex couples that was documented in the main analysis, as well as most additional analyses reported above. Now consider the typical couple that contracts with about ten vendors in the process of planning their wedding, including photographers, bakers, florists, videographers, venues, DJs, bridal/groom salons, calligraphers, jewelers, wedding planners, and more.[138] A conservative estimate of the number of

[137] *Id.* at 28.

[138] Photographers were generally less responsive (to all couples) than other businesses, but the negative effect of sexual orientation was robust across business types.

inquiries would be one per each business category, amounting to ten in total. A more liberal (some might say more representative) estimate assumes that each couple inquires with one or two potential vendors in each category, maybe more, amounting to at least 15–20 encounters. As each vendor-couple interaction presents an independent risk of incurring discrimination,[139] the aggregate risk that same-sex couples would encounter discrimination at least once in their interactions post-*Masterpiece Cakeshop* is a function of the average risk posed by each vendor and the overall number of interactions. This risk ranges from 61% for ten interactions to 85% for twenty interactions,[140] and can go higher (or lower) the more (or fewer) vendors a couple encounters.

3. Does the Effect of *Masterpiece Cakeshop* Vary Between Legal Regimes?

The results demonstrate a substantial reduction in businesses' willingness to provide services to same-sex couples, as compared with opposite-sex couples, after the *Masterpiece Cakeshop* decision. Next, this section asks how this effect displays in different socio-legal regimes. Because of space limitations, the results are summarized in Figure 2. Briefly, I find that *Masterpiece Cakeshop* had a highly statistically significant negative effect in all regimes, *except* for regimes that enacted both an AD law and a religious freedom law.

[139] Clearly, independent vendors in one area could be different than independent vendors in another area, as areas differ in their levels of discrimination. In that sense, the risk posed by each vendor is not entirely independent from the risks posed by neighboring vendors. The *Masterpiece Cakeshop* effect was robust to county-level conservativeness and city size but varied with county-level religious density. On some aspects, then, the assumption of independence holds on average, and on other aspects the risk may vary with the environment. In any event, cases of revealed non-independence were rare and were removed from the sample. *See* Barak-Corren, *supra* note 18, online app. § OA2, at 10–11, https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf, *archived at* https://perma.cc/GF73-AHXU.

[140] In probabilistic terms, the question is: what is the probability that at least one of the vendors will discriminate against the couple, given X vendors and that the average vendor poses a 9% discrimination risk? To answer the question, one needs to calculate the odds that *all* X vendors do *not* discriminate (91% per vendor) and subtract that from 1. P(at least one vendor discriminates) = $1-0.91^X$. This probability is 0.61 for X=10 vendors, 0.76 for X = 15 vendors, 0.85 for X = 20 vendors, and so on.

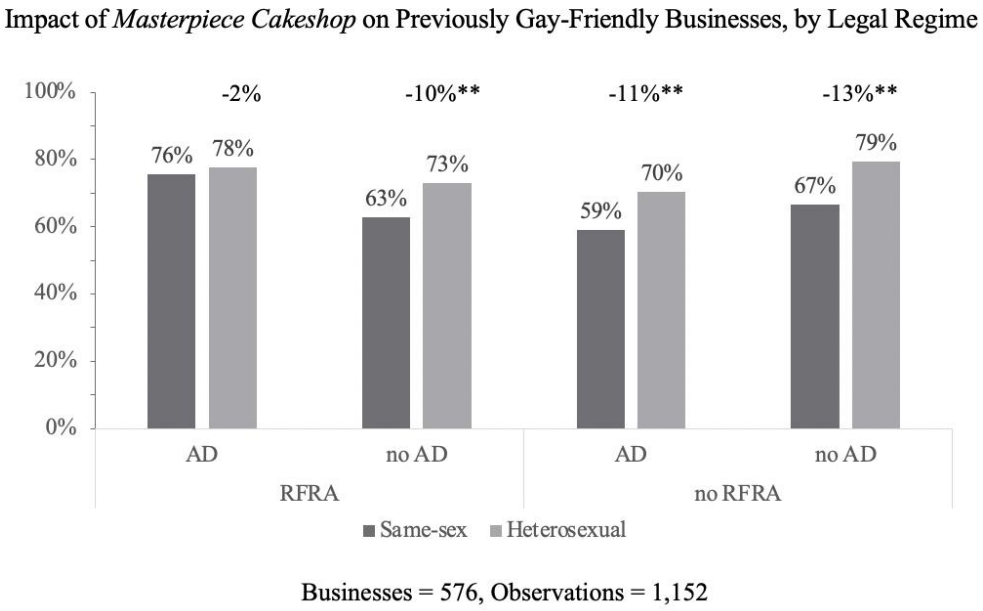

Figure 2. This figure presents the effect of *Masterpiece Cakeshop* on businesses operating in different legal regimes that prior to the decision agreed to provide services to same-sex couples. +RFRA, +AD regimes are counties in Indiana and Texas that are subject to state RFRAs and have enacted local AD laws; +RFRA, -AD regimes are counties in Indiana and Texas that are subject to state RFRAs and have *not* enacted local AD laws; the -RFRA, +AD regime comprises all counties in Iowa, a state that has enacted an AD law and no RFRA; the -RFRA, -AD regime comprises all counties in North Carolina, a state that has not enacted a RFRA and has not enacted an AD law, and had no county with such laws at the time of the experiment. See Table 1 for a socio-demographic comparison of the four regimes. ** $p < .05$; *** $p < .01$.

4. Is the Effect of *Masterpiece Cakeshop* Shaped by Religiosity?

Finally, what role does religion play in business behavior? Given that the decision involves a religious exemption and received considerable attention in religious media, one may expect that businesses operating in more religious environments will be more sensitive to *Masterpiece Cakeshop*, and as a result, the effect will be more pronounced in these environments. This hypothesis is particularly plausible with respect to Evangelical-dominant areas, as Evangelical Christians have been involved in a large number of wedding conflicts and are the denomination with the lowest rates of support for same-sex marriage.[141] Although individual-level evidence on the

[141] PEW RSCH. CTR., U.S. PUBLIC BECOMING LESS RELIGIOUS 108–09 (2015), https://www.pewforum.org/wp-

religiosity of the businesses is unavailable in this study, I can examine the impact of the surrounding religious environment by observing the density of Evangelical congregations in the county where the businesses are located.

I explored this hypothesis using public data on county-level density of religious and particularly Evangelical congregations from the U.S. Religion Census.[142] These data help me examine whether religious environment influences previously gay-friendly businesses after *Masterpiece Cakeshop*.

Figure 3 plots the results. The top panel shows that the gap in agreement to serve same-sex and opposite-sex couples varied with the religiosity of the environment of the businesses. All of the businesses in this analysis agreed to serve same-sex couples before *Masterpiece Cakeshop*. After *Masterpiece Cakeshop*, however, businesses in religiously dense areas showed a large gap between same- and opposite-sex couples. In contrast, businesses in areas with few congregations do not significantly distinguish between same-sex and opposite-sex couples. Plotting the results against the density of Evangelical congregations provides very similar results, as the bottom panel of Figure 3 shows. The data for areas with very few congregations is somewhat noisy (only 32 businesses are located in counties where Evangelical density is 0.0004 or below), yet the general trend is the same: the sexual orientation service gap widens with Evangelical density. Notably, the percentage of businesses agreeing to provide services for opposite-sex couples is fairly stable across high- and low-religious/Evangelical density areas. The fluctuation occurs mostly with respect to same-sex couples.[143]

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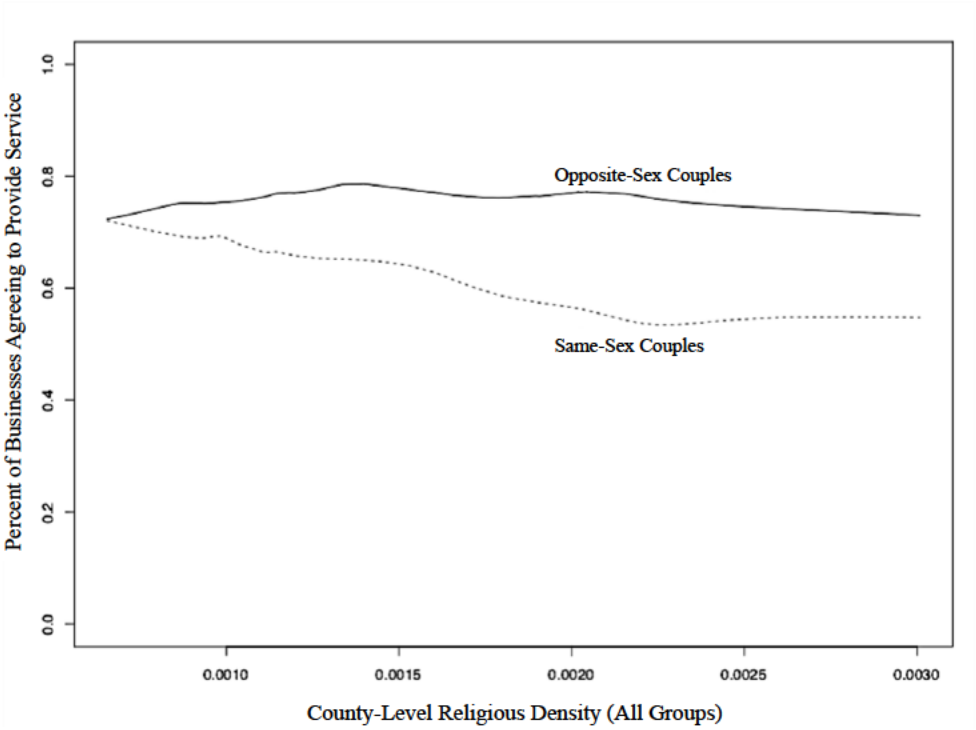
content/uploads/sites/7/2015/11/201.11.03_RLS_II_full_report.pdf, *archived at* https://perma.cc/NW2E-998M.

[142] *See generally* CLIFFORD GRAMMICH ET AL., 2010 U.S. RELIGION CENSUS: RELIGIOUS CONGREGATIONS AND MEMBERSHIP STUDY (2012).

[143] For the results of the regression analyses that account for religious and Evangelical density, see Barak-Corren, *supra* note 18, online app. § OA4.6, at 30–32 tbl.OA4.8, https://harvardcrcl.org/wp-content/uploads/sites/10/2021/05/Online-Appendix-final.pdf, *archived at* https://perma.cc/GF73-AHXU.



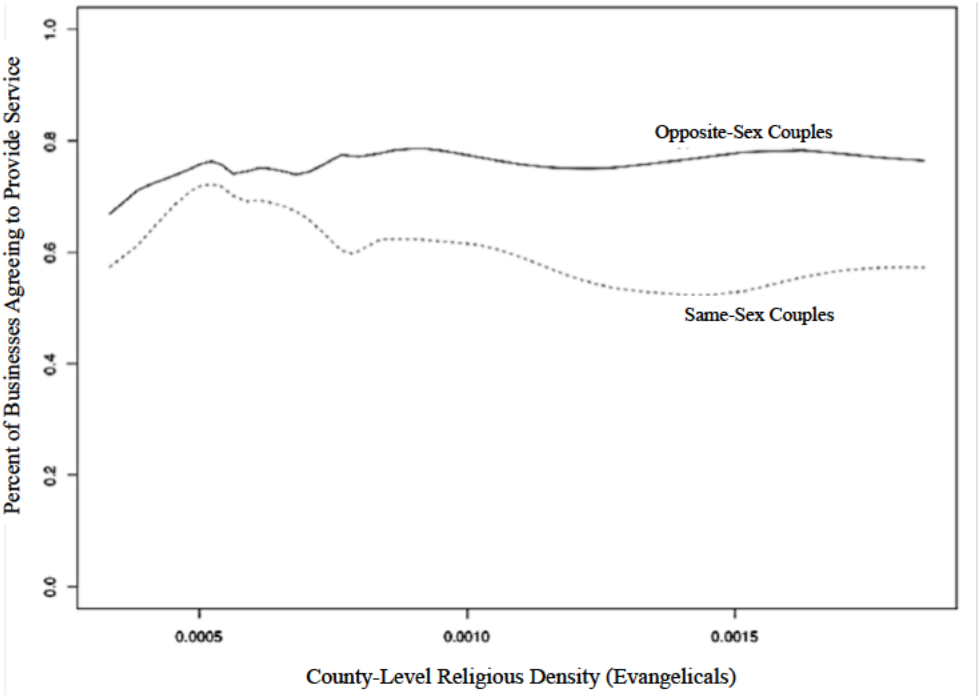


Figure 3. The agreement to serve same-sex and opposite-sex couples post-

Masterpiece Cakeshop by businesses that provided positive responses to same-sex couples prior to *Masterpiece Cakeshop*, as a function of the religious environment. The top panel illustrates the results as a function of congregations density from all religious groups in the county where the business is located. The bottom panel illustrates the results as a function of the density of Evangelical congregations in the county.

These data demonstrate that the negative effect of *Masterpiece Cakeshop* is significantly concentrated in more religious environments. To get a concrete appreciation of the magnitude of this result, I compared businesses in high versus low Evangelical density environments (top 25% versus bottom 25%). In highly Evangelical environments, previously gay-friendly businesses developed a 20.5 percentage point gap between couples (78 percentage points versus 57.5 percentage points),[144] whereas in slightly Evangelical environments, the gap is 2.7 percentage points (70.6 percentage points versus 67.9 percentage points, and non-statistically significant). The same disparity between high and low religiosity areas is true for general religiosity as well.[145] These results are illustrative, yet it is important to note that the effect is not a binary but a continuum. Not only heavily religious (Evangelical) communities show the effect, but also intermediately religious communities, as Figure 3 demonstrates. These results indicate that businesses in more religious areas updated their behavior after *Masterpiece Cakeshop* significantly more than businesses in less religious areas.

III. THE *MASTERPIECE CAKESHOP* EFFECT: EXPLANATIONS AND IMPLICATIONS

The field experiment findings exposed the highly consequential effect of law and the Supreme Court in particular on the behavior of the public. A methodological strength of the field experiment is that it tests the effect of *Masterpiece Cakeshop* directly before and after the decision was rendered, while controlling the setting of the study and employing randomization and is therefore able to isolate the decision's causal effect. It would have been desirable to continue examining *Masterpiece Cakeshop*'s effect later in time, but subsequent legal and political developments have severed the causal link between *Masterpiece Cakeshop* and the market, making such examination impossible. Shortly after the decision, legislatures in several states have

[144] Z = 3.69, p = .0002. Barak-Corren, *supra* note 18, at 36.

[145] A 17.2 percentage point vs. 3.6 percentage point gap, respectively, Z = 3.133, p = .0017. *Id.*

proposed or revived new religious liberty bills[146] and two states surveyed in the experiment—Texas and North Carolina—passed legislation related to religious liberty or LGBTQ rights.[147] Given the constantly dynamic legal and political landscape on these issues, whatever has been the conduct of businesses during the intervening period, it can no longer be linked to *Masterpiece Cakeshop*. The *Masterpiece Cakeshop* field experiment therefore provides the cleanest test of the decision's impact and speaks for the consequences directly stemming from the decision itself.

A. Explaining the Masterpiece Cakeshop *Discriminatory Effect*

What explains the general effect of the *Masterpiece Cakeshop* decision on wedding vendors? Why do they change their behavior after the decision is rendered? Elsewhere, I considered two types of mechanisms that could explain the effect: *cognitive* and *social*.[148]

There are two primary cognitive explanations for the results: a law-and-economics-type explanation and an expressive-law-type explanation. The law-and-economics explanation is that *Masterpiece Cakeshop* was interpreted by vendors as a relief of previously-anticipated penalties for discrimination, or as a signal that the Court has little intention to enforce AD laws. In economic terms, *Masterpiece Cakeshop* may have influenced perceptions regarding the probability of sanction and/or the likelihood of enforcement. The problem with this explanation is that it is less plausible in light of the design of the experiment and its findings. First, for *Masterpiece*

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[146] This includes Iowa and Texas. *See* Rodriguez, *supra* note 100; Emma Platoff, *Texas Senate approves occupational licensing bill LGBTQ advocates call a "license to discriminate"*, TEX. TRIB. (Apr. 2, 2019), https://www.texastribune.org/2019/04/02/texas-senate-religious-refusal-LGBTQ-occupational-licensing/, *archived at* https://perma.cc/J2EQ-GF5H (reporting on S.B. 17, which would allow occupational license holders to cite sincerely held religious beliefs as a defense for license-threatening conduct or speech).

[147] Emma Platoff, *Texas House passes religious liberty bill amid LGBTQ Caucus' objections*, TEX. TRIB. (May 20, 2019), https://www.texastribune.org/2019/05/20/texas-religious-liberty-bill-passes-lgbtq-caucus-fear-hateful-rhetoric/, *archived at* https://perma.cc/5LJF-K2J2; Tim Fitzsimons, *N. Carolina is first in South to ban state funding for conversion therapy*, NBC NEWS (Aug. 3, 2019), https://www nbcnews.com/feature/nbc-out/n-carolina-first-south-ban-state-funding-conversion-therapy-n1038846, *archived at* https://perma.cc/2Y27-4SB3.

[148] Barak-Corren, *supra* note 18, at 36–39.

Cakeshop to relieve the risk of incurring a penalty, such risk should be present to begin with. Yet, the experiment, by design, eliminated the risk of getting caught (by the couple, by society, and by state officials), as emails allow vendors to entirely avoid the detection of discrimination (whereas in face to face communication, the synchronous nature of communication makes it more difficult). Namely, even before *Masterpiece Cakeshop*, vendors could have opted to ignore emails from same-sex couples or provide excuses; they were under no threat of detection, enforcement, or penalty, and *Masterpiece Cakeshop* did not change that. The reputational risk of being labelled a discriminator (and the potential penalty of losing clients) was also absent, for the same reasons. Hence, the decreased willingness to provide services to same-sex couples cannot be attributed to a relief of risk of penalty.

Furthermore, the negative effect of *Masterpiece Cakeshop* is found even in regimes that have not enacted *any* prohibition on the discrimination of same-sex couples (no-AD law regimes). Businesses in Texas, Indiana, and North Carolina, operating under no obligation to serve same-sex couples, and therefore under no threat of sanction, still adapted their behavior post-*Masterpiece Cakeshop*. Hence, it is unlikely that the negative *Masterpiece Cakeshop* effect is explained by the decision's influence on the legal costs of discrimination, even if these costs indeed dropped. Indeed, by no means do I argue that legal or social penalties are inexistent or uninfluential. Both legal penalties and reputational costs can be very influential. However, their absence from the present setting—a common real-life setting where communication is asynchronous and decisions can be easily masked and remain unknown to the public—makes penalties an unlikely explanation for the effect documented in the present study.

An alternative cognitive explanation is that *Masterpiece Cakeshop* had an expressive effect on wedding vendors, changing their perceptions of the social norm regarding service refusal. The expressive theory of law argues that law can foster change not only or merely by the imposition of costs or benefits, but also by conveying that a certain norm has received a consensual status.[149] The *Masterpiece Cakeshop* decision was a lopsided 7–2 ruling that

[149] *See generally* Richard H. McAdams, *An Attudinal Theory of Expressive Law*, 79 OR. L. REV. 339, 339–40 (2000); Cass R. Sunstein, *On the Expressive Function of Law*, 144 U. PA. L. REV. 2021, 2024–25 (1996).

crossed partisan lines and was infused with normative messages. The majority opinion particularly emphasized the importance of tolerance in a free society, the need for pluralism, and respect for the views of religious objectors. These parts of the decision were frequently cited by conservative and religious commentators on the decision.[150] Changes in social norm perceptions and/or personal support of same-sex marriage following the decision could explain why the decision strengthened the impetus of discrimination even if the probability of detection had not changed.[151] Another possibility is that personal preferences did not change, but were emboldened by the expressive message of the decision, making decision-makers more likely to act on them.

Moving from cognitive to social mechanisms, the environment in which vendors operate could have also influenced their decisions. I examined several types of environmental factors: urbanism; conservativeness; and religious density (I also examine different legal regimes, but I discuss this separately). All other things being equal, I did not find that the *Masterpiece Cakeshop* effect weakened in more urban environments, nor strengthened in more conservative environments. But I did find an indication of a more specific social mechanism: the religiosity of the surrounding environment. Businesses in religiously-dense areas discriminated against same-sex couples after *Masterpiece Cakeshop* significantly more than businesses in less religious areas.

Before interpreting these results, it is important to note that the religiosity of the environment is a crude proxy for the role of religion in the decision-making process, a proxy that could interact with additional factors in ways that are not controlled for in the study. Therefore, the findings from this analysis should be viewed as suggestive rather than conclusive. With this in mind, one straightforward interpretation of the results is that areas with more religious congregations have more religious businessowners, and that religious owners are adapting their behavior after *Masterpiece Cakeshop*

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[150] *See supra* notes 89–92.

[151] This explanation is also supported by the evidence on the impact of the Supreme Court on social norms and support of same-sex marriage in *Obergefell v. Hodges*, 576 U.S. 644 (2015). *See* Tankard & Paluck, *supra* note 97, at 1339; Kazyak & Stange, *supra* note 98, at 2044–45.

(likely due to a perceived change in the social norm). Another possibility is that *Masterpiece Cakeshop* received greater exposure and favorable attention in religious environments, such that news of the decision (framed by religious and conservative outlets) spread widely and influenced *all* businesses—religious or not. This hypothesis is supported by the work of Linos and Twist, who found that Supreme Court decisions mostly influence attitudes through one-sided media frames, and that these frames have similar influence on people who regularly consume the relevant media versus those who are randomly exposed to them.[152] Either way, a religious environment appears to play a major role in translating *Masterpiece Cakeshop* into negative consequences for same-sex couples. The precise mechanism by which this translation occurs should be addressed in future studies, including survey experiments to measure individuals' religiosity. Such studies could also broaden the investigation to additional mechanisms and legal measures, such as statutory exemptions.

B. Implications for Legislators

The "legislative mismatch" between the protections of sexual-orientation equality and religious freedom across the country should be a cause for concern on both ends of the political spectrum. The two most common regulatory vehicles to afford such protections—AD laws and RFRAs—have been mostly stalled in recent years due to heightened anxiety about the consequences of AD laws for religious objectors and of RFRAs for LGBTQ people. In May 2019, during a heated debate on the floor of the Texas House about an amendment to the state's RFRA, members of the LGBTQ caucus questioned the bill's sponsors extensively about how the bill might spark discrimination and warned that the bill "perpetuates the rhetoric that leads to discrimination, to hate and ultimately bullying that leads to the consequence of people dying."[153] The last states to enact new RFRAs until recently were Arkansas and Indiana in 2015;[154] the resulting commercial and public

[152] Linos & Twist, *supra* note 86, at 223.

[153] Platoff, *supra* note 147.

[154] NCSL, *supra* note 48. In March 2021, right before this Article went to press, South Dakota became the first state in six years to enact a RFRA. S.B. 124, 2021 Leg., 96th Sess. (S.D. 2021).

backlash might have deterred other states from following that route.[155]

The situation is similar with respect to AD laws. On April 2020, Virginia expanded its public accommodations law to protect against discrimination based on gender identity and sexual orientation.[156] But prior to that, the last state to enact an AD law prohibiting sexual orientation discrimination in public accommodations was Delaware in 2009.[157] Twenty-seven states have not yet enacted such laws.[158]

The *Masterpiece Cakeshop* field experiment conducted a first-of-its-kind examination of the implications of the AD/RFRA mismatch by testing the behavior of wedding vendors from states that are highly similar in terms of their economic, social, and political climates, yet model four different legal regimes: with or without a RFRA, and with or without an AD law. The findings revealed that the introduction of a (perceived) federal religious exemption—in the form of *Masterpiece Cakeshop*—had the same negative impact on same-sex couples in three of the four regimes, but not in regimes that are regulated by both a RFRA *and* an AD law. Intriguingly, the

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[155] *See, e.g.*, James Briggs, *RFRA 'Fix' Was Enough to Keep Tourists Coming to Indianapolis*, DES MOINES REG. (Feb. 3, 2017), https://www.desmoinesregister.com/story/money/2017/02/03/briggs-rfra-fix-enough-keep-tourists-coming-indianapolis/97146094/, *archived* at https://perma.cc/XAS9-QYPT (describing the backlash in Indiana). In Georgia, Gov. Deal vetoed the state's RFRA bill in response to widespread criticism from LGBTQ groups and supporters, including threats of commercial boycott. Greg Bluestein, *BREAKING: Nathan Deal vetoes Georgia's 'religious liberty' bill*, ATLANTA J.-CONST. (Mar. 28, 2016), https://www.ajc.com/blog/politics/breaking-nathan-deal-vetoes-georgia-religious-liberty-bill/yVAFf868i7ilsrwT9zpH3L/, *archived at* https://perma.cc/4C4T-Q59N. The Human Rights Campaign have also made the backlash a salient part of its appeal to states to refrain from enacting RFRAs, for example in its criticism of the recent South Dakota legislation. Wyatt Ronan, *South Dakota Gov. Kristi Noem Signs Religious Refusal Bill, Creating First Major RFRA Law in Six Years*, HUM. RTS. CAMPAIGN (Mar. 13, 2021), https://www hrc.org/press-releases/south-dakota-gov-kristi-noem-signs-religious-refusal-bill-creating-first-major-rfra-law-in-six-years, *archived at* https://perma.cc/PF4H-SPT6.

[156] VA. CODE ANN. § 2.2-3900 (2020).

[157] 77 Del. Laws 90 (2009) (amending 28 sections in the Delaware Code to include sexual orientation).

[158] Out of which, five states—Florida, Kansas, Michigan, North Dakota, and Pennsylvania—recently interpreted the prohibition on "sex" discrimination in their law as including sexual orientation and gender identity. *See State Public Accommodations Nondiscrimination Laws,* MOVEMENT ADVANCEMENT PROJECT (Dec. 10, 2020), https://www.lgbtmap.org/img/maps/citations-nondisc-public-accom.pdf, *archived at* https://perma.cc/PX77-2RZB.

differential effect of *Masterpiece Cakeshop* was partially observed between cities within the same RFRA state that differed on whether they had an AD law or not (e.g., Dallas versus Houston); these differences were associated with significant consequences for discrimination.

Before discussing the potential implications of these results, several caveats are due. To be sure, no causal inferences can be drawn from the legal regime results, as discussed earlier, because legal regimes are not randomly allocated and were impossible to examine in a natural experiment setting in the present context. Therefore, I am not arguing that the (in)existence of one law or the other is the cause for the *Masterpiece Cakeshop* effect. In addition, legal regimes are considerably richer and more nuanced than the letter of the law can reveal; and they are influenced, among other factors, by administrative policies and judicial decisions not captured in this analysis. Furthermore, legal differences between otherwise similar political units could be the result of unobservable variables that could be the actual causes of differences in discrimination. For example, the social and political climate that produced certain legislation might *also* shape the conduct of local businesses; such an explanation is probably more likely than the assumption that wedding businesses are fully familiar with the laws of their political units.

The underlying causes of the findings aside, the results carefully suggest two observations about the implications of the legislative mismatch. First, AD laws do not necessarily safeguard sexual orientation equality or protect against an increase in discrimination. Second, RFRAs are not necessarily themselves detrimental to the operation of equality on the ground.

1. The Push for Federal and State AD Laws Should Not Forsake Local AD Laws.

That AD laws do not necessarily ensure equality is not, on its own, novel. Extensive empirical research has repeatedly exposed and documented the failures of AD laws to prevent and remedy discrimination in practice.[159] Yet it is interesting to observe that regimes that enacted an AD law, but no RFRA,

[159] For a review based on comprehensive data, see ELLEN BERREY ET AL., RIGHTS ON TRIAL: HOW WORKPLACE DISCRIMINATION LAW PERPETUATES INEQUALITY 54–73 (2017).

fare *worse* than comparable regimes that enacted both laws. Iowa, for example, has a long tradition of protection and advancement of sexual orientation equality. Iowa led the way for other states in invalidating its sodomy law already in 1976 and being one of the first states to recognize same-sex marriage.[160] The state enacted a state-wide ban on sexual orientation discrimination, and efforts to enact a RFRA in Iowa failed several times due to concerns about the potentially detrimental effects of such act on sexual orientation discrimination.[161] Against this background, one could expect that the social and political climate that produced Iowa's legal regime would be the most favorable to same-sex couples of all four regimes. Instead, business behavior in Iowa is found to be indistinguishable from regimes that have neither an AD law nor a RFRA (North Carolina), or even from regimes that have no AD law, but do have a RFRA (certain localities in Texas and Indiana). In contrast, regimes that have both an AD and a RFRA (other localities in Texas and Indiana) did not show the negative *Masterpiece Cakeshop* effect.

This pattern raises the question of whether AD laws vary in their effectiveness based on the level of their enactment—namely, whether municipal AD laws are more effective than state variations. This possibility runs counter to the intuition of LGBTQ advocacy groups in many AD-less states. Some of these groups intensified their struggle for state-level AD legislation following *Masterpiece Cakeshop*, claiming that municipal legislative acts are insufficient and "do not carry the force that a state law would."[162] Clearly, enacting a series of municipal ordinances is less efficient than enacting one law that covers all municipalities and provides legal recourse for the entire state population. Yet there are two potential reasons for why local AD legislation may fare better in reducing discrimination than state-level legislation. First, legislation at the local level may better represent the preferences and behavioral intentions of the political community.[163]

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[160] *See* sources cited *supra* note 100 and accompanying text.

[161] *See Iowa Religious Freedom Restoration Act (HF 258)*, REWIRE NEWS GROUP (Feb. 19, 2019), https://rewire.news/legislative-tracker/law/iowa-religious-freedom-restoration-act-hf-258/, *archived at* https://perma.cc/22MK-GG7M (documenting the failure of several bills in 2016 and 2018 and the stalling of a 2019 bill).

[162] *See* Platoff, *supra* note 87.

[163] For a discussion of how cities promote democratic self-governance and representation better than states, see Yishai Blank, *City Speech*, 54 HARV. C.R-C.L. L. REV.

Therefore, the enactment of a municipal AD law by a certain community likely provides a more reliable commitment to equality and nondiscrimination than the enactment of a state AD law. Second, and relatedly, because municipal legislation is closer to home, it could be more successful in persuading residents that have not yet bought into the norm. According to the expressive theory of law, "[a]s long as legislation is positively correlated with popular attitudes or opinions, then it will cause individuals to revise their beliefs about the expected approval or disapproval and to act accordingly."[164] If this proposition holds in the present case, the fact that a municipal AD law represents the norm of the immediate community increases its ability to persuade individuals from that community to conform. This ability could be compromised the higher a certain legislation "climbs" (namely, a state law might succeed less in revising behavior than municipal law, and federal law might have even less success). This decrease in effectiveness is especially likely in diverse states, where communities that adhere to different norms could respond to AD laws very differently.[165]

Given that the findings with respect to state differences are correlational and may therefore reflect a variety of additional factors, these conclusions are tentative and should be further examined in future studies.

One implication for the interim period is not to abandon local initiatives to enact AD laws or prioritize them as less urgent or less important than state-level initiatives. Assuming that equality movements care not only about the law on the books, but also (and perhaps more so) about the law on the ground, including the prevention of actual discrimination and the improvement of people's lives and opportunities, local AD laws appear to contribute greatly to achieving these goals.

365, 389–96 (2019).

[164] McAdams, *supra* note 149, at 343; *see also* Robert Cooter, *Expressive Law and Economics*, 27 J. LEGAL STUD. 585, 595 (1998) (hypothesizing that enacting a norm can increase the number of people who follow it).

[165] In such cases opposing communities could react against the law. *See* Netta Barak-Corren et al., *The Provocative Effect of Law: Majority Nationalism and Minority Discrimination*, 15 J. EMPIRICAL LEGAL STUD. 951, 956 (2018).

2. RFRAs Are Not Necessarily Recipes for Discrimination and Should Be Pre-Tested to That Effect.

The second important finding that emerges from the comparison of legal regimes is that RFRAs are not necessarily detrimental to the operation of sexual orientation equality. This finding is arguably more surprising and potentially of broad relevance. The enactment of RFRAs and other protections of religious liberty has been the focus of intensive debate in recent years: one of the major concerns being that such laws would encourage greater discrimination against sexual minorities. I already alluded to the levels of anxiety and controversy that characterize this issue. States that enacted or considered enacting RFRAs were threatened with high-impact boycotts. Indiana itself was the subject of such a boycott after passing its RFRA in 2015, losing twelve conventions and $60 million in revenue.[166] The Indiana legislature quickly passed a "fix" that clarified that the new Act does not trump local AD laws,[167] a provision very similar to the one included in Texas' RFRA from its inception.[168]

The results from the *Masterpiece Cakeshop* field experiment indicate that the combination of religious liberty protections of the Texas-Indiana type with AD laws at the local level was resistant to the negative effect of *Masterpiece Cakeshop* on discrimination towards same-sex couples. One potential explanation is that the tension built into these hybrid regimes led businesses to reflect and contemplate their positions in advance—prior to *Masterpiece Cakeshop*—more, perhaps, than businesses in regimes where the tension was less salient. Having already formed a position, businesses in hybrid regimes were possibly more resistant to the influence of *Masterpiece Cakeshop*.[169] Notably, these businesses were not merely more consistent in their behavior; they were also the least discriminatory of same-sex couples post-*Masterpiece Cakeshop* (see Figure 3). Seventy-six percent of businesses in hybrid regimes agreed to provide services to same-sex couples, compared to 59–67% of businesses in other regimes.

As with the findings regarding AD regimes, the relationship between

[166] Briggs, *supra* note 155.
[167] *See supra* notes 64–65.
[168] *Id*.
[169] I thank Stephanie Barclay for proposing this point.

hybrid regimes and sexual orientation discrimination should be further examined. In particular, RFRAs come in many shapes and forms—e.g., with or without recourse against local governments, private lawsuits, and civil rights law.[170] Different RFRA designs could have different impacts on discrimination, especially as these designs interact with existing or inexistent AD laws. To be sure, businesses in RFRA regimes without AD laws strongly showed the negative *Masterpiece Cakeshop* effect. Caveat is required before enacting a new RFRA or amending an existing act.

Alongside this caveat, the findings regarding hybrid regimes provide tentative hope for scholarly and political efforts—most notably, Professor Robin Fretwell Wilson's work—that marriage equality and religious liberty could be reconciled in legislation somehow, without necessarily exacerbating discrimination.[171] How might this goal be achieved?

An important implication of the *Masterpiece Cakeshop* experiment is that such efforts should rely on reliable and robust empirical evidence regarding the likely consequences of the proposal on sexual orientation discrimination. To do that, I propose pre-testing RFRAs (and any other similar mechanism). Lawmakers and law professors must not speculate on the outcomes of their proposals or treat them as self-evident. As the findings of the field experiment teach us, speculations and assumptions that do not rely on directly relevant data are no good. The discipline of empirical legal studies has advanced to offer a variety of methods—including experimental surveys and qualitative in-depth interviews—that could facilitate testing the likely effects of proposed policies before formal implementation.[172]

For example, a state legislature could collect a representative sample of the state population, randomly expose different groups to alternative bills, and examine whether exposure to one bill (compared to the others, or no bill)

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[170] *See supra* notes 57–64 and accompanying text.

[171] *See, e.g.*, Robin Fretwell Wilson & Anthony Michael Kreis, *Embracing Compromise: Marriage Equality and Religious Liberty in the Political Process*, 15 GEO. J. GENDER & L. 485, 488–89 (2014). Notably, Wilson and her colleague do not promote new RFRAs, but instead specific and clear exemptions for wedding vendors from the duty to provide service to same-sex couples (but not other suspect categories). *Id.*

[172] In general, the field of evidence-based lawmaking has developed both theoretically and practically in recent years. *See* Ittai Bar-Siman-Tov, *The Dual Meaning of Evidence-based Judicial Review of Legislation*, 4 THEORY PRAC. & LEGIS. 107, 108–11 (2016).

generates more or less anti-gay bias in the population, or produces a more or less accurate understanding of appropriate behavior. Lawmakers could either devise their own decision-making dilemmas to probe citizens' understanding of the proposed law, or they could rely on one of the many measures established in psychological research to capture bias and social norm perceptions.[173]

Clearly, pre-testing laws requires collaboration between lawmakers and empirical legal scholars, or even the establishment of an in-house research department to execute empirical studies for legislatures. Yet the benefits of such approach greatly exceed its costs. First, basing legislation on data, rather than on speculations, is a positive good which improves the quality of the legislative process. Second, the fears and anxiety that accompany the religion-equality conflict prevent the advancement of both AD laws and RFRAs all around the nation and exacerbate cultural divides and political polarization. Were the opposing parties to suspend their assumptions about the consequences of proposed policies and subject them to a rigorous empirical test, they might have been able to approach proposals more openly. In addition, the interim phase of subjecting bills to an a-priori empirical test, before legislating them, will facilitate bipartisan collaboration in research design. Pro-religion and pro-equality legislators will have to sit down and decide what bills they want to test and what measures are needed to capture the consequences they fear or favor, if real. For example, they will need to jointly draft the vignettes (or scenarios) they are interested in probing citizens' reactions to. This deliberation could clarify the stakes for both parties, encourage more reflection about their goals and concerns, and concretize the debate going forward. The results would hopefully resolve the debate in one direction or the other and provide informed ground to base any decision regarding the legislation.

C. Implications for Courts

The findings of the *Masterpiece Cakeshop* field experiment answer several legal questions currently preoccupying the courts.

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[173] *See, e.g.*, Ofosu et al., *supra* note 99, at 8847–48; Tankard & Paluck, *supra* note 97, at 1336–39.

First, courts are the arbitrators of the debate on the consequences of religious exemptions. Complainants of discrimination and supporting amici frequently warn of increased discrimination towards same-sex couples if religious exemptions are granted. Religious objectors and supporting amici consistently argue that this concern should be dismissed because "ample alternative providers exist."[174] As a result, courts ask what the consequences of their decisions are likely to be—as did Justice Kennedy, who penned the majority opinion in *Masterpiece Cakeshop*.[175] But, until now, courts have had no relevant data to answer this question.

The *Masterpiece Cakeshop* field experiment provides these data for the first time, documenting the scope of refusals to same-sex couples, as compared with opposite-sex couples, in response to the *Masterpiece Cakeshop* decision. Courts now have concrete evidence from different legal regimes in the U.S., data that were thus far the object of concerns and speculation. Importantly, these data are not drawn from liberal strongholds, but from states that are either at the national average or more conservative. The data show courts that market alternatives *do* exist—there are vendors who will provide services to same-sex couples—*and* that granting a religious exemption encourages discrimination towards same-sex couples nevertheless, across a wide range of social and legal categories. Indeed, the *Masterpiece Cakeshop* decision generally exposed same-sex couples to a high risk of experiencing discrimination—estimated to be between 65% and 81%, as a function of the number of market interactions in which couples engage. Justice Kennedy's concern that an exemption would encourage wedding vendors to refuse service to same-sex couples is unfortunately borne out by the data.

These troubling consequences provide the missing piece to the puzzle of applying a strict scrutiny analysis, or RFRA review, to AD laws. Under this doctrine,[176] the court first examines whether the law substantially burdens the

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[174] Laycock & Berg, *supra* note 76; *see also* Transcript of Oral Argument, *supra* note 12, at 46 (U.S. Attorney General arguing in support of the baker that "products are widely available from many different sources").

[175] *Id.* at 44–45.

[176] In 29 states, the standard of scrutiny of governmental burdens on religion is currently lower, encompassing standards such as intermediate scrutiny or even rational basis review. *See supra* Part I.B.

free exercise of religion. To survive strict scrutiny, a law must be the least restrictive means by which to further a compelling governmental interest. This is a high threshold for the government, one "that takes a hard look at the facts and does not accept the 'government's bare say-so' about factual outcomes."[177] The first part of the justification—proving a compelling governmental interest—is typically uncontroversial when it comes to laws that aim to fight discrimination on the basis of sexual orientation. But the second part—the least restrictive means test—is thornier, especially in cases involving potential religious exemptions. Cannot the compelling governmental interest in eradicating discrimination be achieved through the less restrictive means of a law that permits religious exemptions? To determine whether religious exemptions would undermine a law's purpose, judges must engage in a factual examination of the consequences of religious exemptions. To know whether a universal enforcement of AD laws is the least restrictive means to ensure access to public accommodations, courts need to know whether religious exemptions detract from this compelling goal. The results of the *Masterpiece Cakeshop* field experiment establish that the decision substantially detracted from this goal in most regimes, by substantially expanding discrimination against same-sex couples. Unfortunately, this was the outcome even though the Court reiterated its commitment to protecting sexual minorities in the marketplace in general, and despite the fact that the exemption crafted for the baker was narrow and case specific. Taken together, these results vindicate states that currently insist on enforcing AD laws without providing exemptions. As I note earlier, it is possible that a different combination of legal means will generate different behavioral outcomes, and such combinations should be tested—or, where relevant, pre-tested—in the appropriate circumstances in the future.

The second implication for courts involves the specific reasoning of the *Masterpiece Cakeshop* decision. The majority Justices—particularly Justice Kennedy—clearly wished to avoid settling the larger tension between religious liberty and sexual orientation equality. Instead, the Justices sought to carve a narrow decision that would not grant wedding vendors a license to discriminate against same-sex couples. This strategy does not seem to have

[177] Stephanie Barclay, *An Economic Approach to Religious Exemptions*, 72 FLA. L. REV. 1211, 1262 (2020) (quoting Yellowbear v. Lampert, 741 F.3d 48, 59 (10th Cir. 2014)).

been successful. *Masterpiece Cakeshop* has ultimately increased discrimination in the wedding industry and bolstered legislators and advocates in their attempts to expand religious protections and narrow the scope of antidiscrimination protections.[178] The two subsequent unreasoned decisions in *Arlene's Flowers*[179] and *Klein*[180] that vacated and remanded other wedding vendor cases, despite very different factual circumstances, might have strengthened the impression that *Masterpiece Cakeshop* was not so narrow after all. Assuming the Court did not intend to expand discrimination against same-sex couples, could other judicial strategies have fared better?

This question is of crucial importance considering the challenge facing the Court in its coming term, and likely future ones. This term, the Court will decide *Fulton v. City of Philadelphia*, which involves a social welfare agency that objects, on religious grounds, to a Philadelphia AD rule. That rule requires the agency to provide services to LGBTQ prospective foster parents, as part of its governmental contract. The agency argues that if the Court rules for the city, the agency will close its doors and children will be harmed. The city argues that same-sex couples should not be excluded solely on the basis of their sexual orientation, and that all children are better off if placement agencies refrain from considering factors other than the best interest of children.

The *Masterpiece Cakeshop* experiment offers two lessons for the *Fulton* Court—and for any court adjudicating religion-equality conflicts in the future. First, the Court should require the parties to present directly relevant data to found arguments about consequences. The findings from the *Masterpiece Cakeshop* field experiment teach us that contrasting arguments about the empirical world can thrive despite the absence of data, as each party to the debate is highly motivated to hold on to their own assumptions and to speculate about the facts. This is a dangerous state of affairs. In constitutional law, as elsewhere, arguments about outcomes should rest on actual data. Not

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[178] *See* Simpson, *supra* note 87 (quoting the head of the American Family Association of Indiana saying that he sees *Masterpiece Cakeshop* as a "signal" to push forward litigation against local AD laws in Indiana); Platoff, *supra* note 147 (describing bills in Texas expanding protections for religious professionals and corporations).

[179] Arlene's Flowers, Inc. v. Washington, 138 S. Ct. 2671, 2671–72 (2018).

[180] Klein v. Or. Bureau of Lab. & Indus., 139 S. Ct. 2713, 2713 (2019).

only are data crucial to learn the truth value of consequentialist arguments, they can also nuance and refine legal analysis. In the *Masterpiece Cakeshop* context, the empirical work exposes issues that were not previously considered in the area of religious exemptions—such as the nature of the influence of law on behavior—and creates new and specific questions for lawyers and judges to answer—such as how to factor the aggregate risk of discrimination. It is therefore imperative that courts will develop a more critical approach to consequential arguments and will look for directly relevant data.

The second lesson for the *Fulton* Court flows directly from the poor outcomes resulting from the avoidance strategy used in *Masterpiece Cakeshop*. By now, several different studies—including the present study—have shown that the Court has the power to shape public attitudes and public behavior, thereby producing either less or more bias and discrimination in society.[181] It is true that after a decision is handed down by the Court, it takes on a life of its own, and much of its effects depend on how it is communicated by mass media. But the Court is not a helpless statist in this process. A clearer and less ambiguous decision—for example, one that sets a clear rule that is easy to communicate, understand, and follow—is less open to aggrandization, misstatements, or misinterpretations. The *Fulton* Court should opt for a clear and bright-line decision that provides specific and unambiguous behavioral instructions, including for future cases. This is particularly true if the court decides to grant an exemption in *Fulton*. In such case, the Court should assume, based on the *Masterpiece Cakeshop* effect, that its decision will likely encourage discrimination against sexual minorities. It is the Court's responsibility to minimize this effect to the extent possible. The Justices should not mislead themselves to think that evading the big questions or making a case-specific decision, as in *Masterpiece Cakeshop*, will avoid undesirable outcomes. The Justices should also not mislead themselves to think that their decision will only expand the freedom of a negligible minority of extremely objecting individuals. Rather,

[181] *Cf.* Ofosu et. al, *supra* note 99, at 8849 (finding a sharper decrease in anti-gay bias in states that legalized same-sex marriage compared with those that did not); Tankard & Paluck, *supra* note 97, at 1341–42 (finding that the Supreme Court's decision in *Obergefell v. Hodges*, 576 U.S. 644 (2015), shifted perceived social norms among non-LGBTQIA Americans in support of same-sex marriage).

exempting religious objectors will likely have a broad impact, including on decision-makers who were willing to provide services before the decision, but will refuse to do so afterwards. These consequences are particularly concerning given the number of wedding conflict cases that have recently resolved in favor of vendors.[182] This trend gives rise to the possibility that the *Masterpiece Cakeshop* effect will repeat and aggregate over time, the more such decisions are made and become known to the public.

Finally, the expansion of discrimination post-*Masterpiece Cakeshop* suggests that courts should develop a better account of the burden that AD laws place on religious objectors. The dominant theory of the relationship between religious exemptions and religious objection put forth in litigation is that religious exemptions relieve the harm that antidiscrimination rules inflict on religious individuals. Among other things, the theory assumes that the only effect of exemptions would be to relieve devout individuals of the societal harm, but that exemptions do not change behavior, because religious objectors would not have provided services to same-sex couples in any event.[183] Recently, Professor Barclay suggested to formalize this theory in economic terms, arguing that the harms incurred by same-sex couples (as a result of discrimination) should be weighed against the harms incurred by religious objectors (as a result of the AD law), by examining the transaction costs for each party. Barclay argues that the costs to religious objectors are extremely high, because of the idiosyncratic and fixed nature of their beliefs. She reasons that compelling them to act against their faith will increase net societal harm, because religious objectors will either become martyrs, or will end up with a broken conscience.[184] Under this theory of Berg, Laycock, Barclay, and others, the availability of exemptions should not change the scope of religious objection (because they will not enter the transaction in any event), only its consequences for the objectors.

But the results of the *Masterpiece Cakeshop* experiment bely this theory. First, the seeming availability of a religious exemption post-*Masterpiece*

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[182] *See* Telescope Media Grp. v. Lucero, 936 F.3d 740, 762 (8th Cir. 2019); Brush & Nib Studio v. City of Phoenix, 448 P.3d 890, 926–27 (Ariz. 2019); Lexington-Fayette Urban Cnty. Hum. Rts. Comm'n v. Hands On Originals, 592 S.W.3d 291, 298 (Ky. 2019).

[183] Berg & Laycock's Brief, *supra* note 79, at 32.

[184] Barclay, *supra* note 177, at 27–28 (relying in part on Christopher Lund, *Martyrdom and Religious Freedom*, 50 CONN. L. REV. 959, 965–67 (2018)).

Cakeshop changed the scope of refusal to same-sex couples. To the extent that this effect is due to *Masterpiece Cakeshop*'s encouragement of religiously motivated objection, the data unsettle the theory that religious objection is a result of permanent idiosyncratic features of the objectors' religious identity, features that are unyielding to external influence. Rather, it seems that religious objection is contingent on the seeming availability of an exemption. The demand for objection is not fixed, but elastic. Second, this effect is not related to the imposition or relief of any penalty or enforcement. While it is theoretically possible that prior to *Masterpiece Cakeshop*, some religious objectors in no-exemption regimes caved in to legal pressure because they could not afford the penalties,[185] the experiment shows that wedding vendors changed their behavior in the absence of any state penalty and absent any likelihood of enforcement. First, as discussed above, discrimination increased in regimes that do not prohibit discrimination at all. Second, the option to ignore an email from a same-sex couple was available to all vendors both before and after *Masterpiece Cakeshop*, without anyone ever knowing their reasons for doing so. Wedding vendors changed their behavior not because *Masterpiece Cakeshop* relieved them of a penalty associated with their behavior, but due to other reasons—more likely, the expressive effect of the decision.[186]

These findings suggest that transaction costs in religion-equality conflicts are in fact dynamic, and that the religious objection to AD laws can fluctuate as a result of the availability of exemptions in ways that defy the "martyr/broken conscience" dichotomy.[187] These findings require courts to probe deeper into the characteristics of religious objection and explore more carefully the assumptions regarding the magnitude of harm caused to religious objectors from the *un*availability of exemptions.

Clearly, this is a highly sensitive issue, and posing the question by no means underestimates the possibility that such harm is real and grave for some religious objectors. At the same time, law in general and the Supreme Court, in particular, always navigate two different levels of generality: the

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[185] Despite the conscientious harms they experienced. This is the "broken conscience" concern developed in the context of harms by Barclay, *supra* note 177, at 27–28.

[186] *See supra* Part III.A.

[187] Barclay, *supra* note 177, at 27–28.

specific case and the general rule. In specific cases involving specific objectors, the harm from not providing an exemption could be enormous. Yet, because each decision also contributes to the formation of a general rule, courts cannot ignore how specific decisions eventually create precedents that influence the availability of rights and remedies for everyone, including individuals who do not necessarily share the features of the specific objector. The *Masterpiece Cakeshop* effect indicates that there are wedding vendors in this broader category who are willing to provide services to same-sex weddings in the first instance but become unwilling to do so once an exemption is announced.

There is no doubt that the Court faces an acute dilemma. Both equality before the law and religious liberty are fundamental constitutional rights, and setting their respective boundaries is no simple task. However the Court decides to resolve the constitutional issues at hand, it ought to consider the harms that might result from its decision, and to avoid or mitigate these harms if possible. Courts are often motivated by a desire to provide justice in particular cases without creating inadvertent and unjust consequences across the board. An important first step towards this goal is to follow the general prescription offered in this Article: to rest constitutional analyses of consequentialist arguments directly on relevant empirical evidence.

EXHIBIT C

50(1) THE JOURNAL OF LEGAL STUDIES 1 (in press)

Religious Exemptions Increase Discrimination Towards Same-Sex Couples: Evidence from Masterpiece Cakeshop

Netta Barak-Corren[*]

ABSTRACT

In 2018, the Supreme Court decided *Masterpiece Cakeshop v. Colorado Civil Rights Commission* in favor of a baker who refused service to a same-sex couple due to his religious beliefs. This article examines the behavioral effect of this decision in an experiment (N=1,155 businesses) that measured discrimination towards same-sex couples in wedding services shortly before and after *Masterpiece*. I find that *Masterpiece* significantly reduced the agreement to serve same-sex couples as compared with opposite-sex couples, even among previously willing vendors. Considering the variety of vendors involved in a typical wedding, I estimate the odds that same-sex couples would experience discrimination post-*Masterpiece* between 61% and 85%. These results show that even a narrowly construed exemption can have a significant and robust, even if inadvertent impact on a market and its customers. I discuss the implications of these results for the research on Supreme Court effects on the public.

1. INTRODUCTION

What are the consequences of exempting religious objectors from antidiscrimination law?

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Answering this question is crucial to resolving the escalating tension between religious freedom and sexual orientation and gender equality in the United States. So far, the primary legal tool that has been proposed as a solution has been religious exemptions. But the use of exemptions have also raised considerable objections. Central among those is the concern that granting exemptions would escalate the number and significance of religious claims and extend LGBTQ discrimination (NeJaime 2012; NeJaime and Siegel 2015; Stern 2014). Proponents of religious exemptions reject these concerns as factual nonsense, arguing that religious objectors are a negligible minority in a society growing ever more affirming of LGBTQ equality, and that exempting religious objectors will not expand discrimination against same-sex couples (Koppelman 2014; Berg and Laycock 2017; Laycock and Berg 2018; Laycock 2017, 3:962).

Currently, there is almost no evidence that could clarify which of the contradictory factual arguments is actually true. Not only that such evidence is required to settle and refine theoretical debates, it is also crucial to inform legislators debating whether to enact religious exemptions, and courts deliberating whether to grant such exemptions. From *Reynolds v. United States* (1878), the first case to bring the question of religious exemptions before the Supreme Court, through key decisions such as *Employment Division v. Smith*[1] and *Burwell v. Hobby Lobby Stores, Inc.*,[2] up to *Masterpiece Cakeshop, Ltd. v. Colorado Civil Rights Commission*[3]--the decision at focus in the present study—the Supreme Court has always cited the social consequences of exemptions (or

[1] 292 U.S. 872 (1990) (Scalia, J.) (Citing Reynolds v. United States (1878): "To permit this would be to make the professed doctrines of religious belief superior to the law of the land, and in effect to permit every citizen to become a law unto himself.")

[2] 134 S. Ct. 2751, 2782 (2014) (Alito, J.) (resting the majority opinion on the assumption that the exemption's effect on third-parties "would be precisely zero").

[3] 138 S. Ct. 1719 (U.S. 2018).

lack thereof) when determining whether to reject (or grant) petitions for religious exemptions.

This article elucidates the consequentialist debate on religious exemptions by studying, for the first time, the effects of religious exemptions on sexual orientation discrimination. Part 2 begins by presenting the necessary legal background and the *Masterpiece* decision, in which the Supreme Court ruled in favor of a religious baker who refused service to a same-sex couple. Next, Part 3 describes a large-scale experiment that I designed to measure the impact of *Masterpiece* on sexual orientation discrimination in the wedding services market. To this end, I combined methods from pseudo-experiments (studies that examine the impact of a reform by focusing on events occurring shortly before and after the reform) and field-experiments (studies that randomly allocate different treatments to subjects in the field). Wedding vendors (bakers, photographers, and florists) were sampled to the experiment from the four legal regimes currently existing in the United States that differ based on whether they prohibit sexual orientation discrimination in public accommodations (AD law) or not, and on whether they facilitate religious exemptions via a Religious Freedom Restoration Act (RFRA) or not.[4] This resulted in a 2 (AD law/no AD law) by 2 (RFRA/no RFRA) matrix from which 906 businesses were sampled to the experiment. Each business was examined shortly before (May 8th-15th, 2018) and after (June 13th-20th, 2018) the *Masterpiece* decision (rendered on June 4th, 2018). In each period, wedding businesses were contacted via email by a same-sex or an opposite-sex couple inquiring about wedding services. Each business was contacted by the two types of couples both before and after the decision, resulting in four observations per business and a rich dataset that allows for both within-business and across-businesses comparisons. A "control" group of 251 businesses was contacted for the first time after

[4] RFRAs are not the only legal vehicle facilitating religious exemptions. Others are addressed in Part 2.

Masterpiece, by both couple types, following the same procedures. The question of interest was whether businesses agreed to provide the requested service to couples, and specifically whether *Masterpiece* influenced the responses businesses provide to same-sex couples.

Part 4 presents the results of the *Masterpiece* experiment. Briefly, the decision significantly reduced the willingness to serve same-sex couples, from 63.6% before *Masterpiece* to only 49.2% after the decision was rendered (a 14.4 percentage-point gap, or ~23 percent decrease in favorable responses). Zooming in on businesses that, prior to *Masterpiece*, responded positively to same-sex couples, I find that these businesses discriminate between opposite-sex and same-sex couples after *Masterpiece*. Previously "gay-friendly" businesses that are randomly contacted by opposite-sex or same-sex couples after the decision was rendered respond less favorably to same-sex couples (75.5% vs. 66.3%, a 9 percentage-point gap, or 12 percent fewer favorable responses). This effect is not an artifact of the experiment itself, as it is identically found in the "control" group. Probing into the differences between the four legal regimes, I find that the negative *Masterpiece* effect appears in all regimes, except for those that enacted *both* an AD law and a RFRA. The effect is robust, including in analyses that control for county-level conservativeness and analyses limited to businesses located in big cities (where, it is often argued, there is no discrimination problem). However, the effect of *Masterpiece* is significantly more pronounced in religious environments, as proxied by the density of congregations in the county where the business is located.

A back-of-the-envelope calculation demonstrates the broader implications of these results. Provided that couples of all identities typically contract with about ten types of vendors in the process of organizing a wedding (reception venues, wedding planners, bakers, florists, photographers, videographers, bridal/groom salons, jewelers, DJs, and calligraphers—a partial

list); and that they often inquire with several vendors of each category; and that the average risk of experiencing discrimination post-*Masterpiece* is about 9%; I estimate the aggregate risk of discrimination for same-sex couples between 61% and 85%. This means that, across the observed differences between legal regimes and religious environments, *Masterpiece* has the general effect of exposing same-sex couples to heightened risk of discrimination in the organization of weddings.

Part 5 discusses the results. First, I consider potential explanations for the effect of *Masterpiece* on vendors' behavior, examining both cognitive mechanisms (how the decision is perceived and factored into the decision-making process) and social mechanisms (how social factors, particularly religion, interact with the decision-making process). Moving to implications, I discuss the contributions of the study to the research of Supreme Court effects on the public. In particular, the study sheds new light on the argument that courts are ineffective at spurring social change (Rosenberg 2008), by highlighting the risk that the court will emerge as effective, paradoxically, by spurring inadvertent social change. On the constitutional end, the results discredit the argument that religious exemptions will not expand discrimination. Instead, what the *Masterpiece* experiment shows is that even a narrow exemption can have a significant and robust impact on a market and its customers. Next to these implications, the observed variation between legal regimes and religious environments provides preliminary evidence that discrimination can be minimized under certain conditions. These findings can guide future research on religion-equality conflicts, Supreme Court effects, and the dynamics of discrimination more broadly.

2. LEGAL BACKGROUND

A. The "legislative mismatch" of Antidiscrimination and Religious Freedom Laws

The regulation of sexual orientation discrimination and religious freedom in the U.S. vary at the state and local level. Some jurisdictions prohibit public accommodations (such as wedding businesses) from discriminating on the basis of sexual orientation (hereinafter: "AD laws"), while other jurisdictions do not prohibit such discrimination.[5] Concomitantly, some jurisdictions enacted rules that facilitate the creation of religious exemptions from rules of general applicability (often these are Religious Freedom Restoration Acts, or RFRAs), while other jurisdictions have not enacted such rules.[6] With the legalization of same-sex marriage, new RFRAs were enacted in Mississippi (2014), Indiana, and Arkansas (NCSL 2017). In other states, e.g., Iowa and Georgia, RFRA bills failed due to concerns about their implications for LGBTQ rights and fears from commercial boycotts (Foody 2015). In the process, RFRAs became the legislative antonym of AD laws (Ferguson 2015).

The distribution of AD laws and RFRAs across states is a "legislative mismatch" (Lupu 2015) with a narrow overlap. The overlap consists of four states that enacted both laws,[7] a maximum of seven states that have enacted AD laws and have extended protections on religious freedom in their constitutions,[8] and a considerable number of local governments in RFRA states that enacted municipal AD laws (Lupu 2015, pp. 48-49). This last category includes a number of major cities

[5] Twenty-two states, the District of Columbia, and numerous local governments enacted such laws (HRC 2018).

[6] Twenty-one states enacted RFRAs (NCSL 2017) and ten states interpreted their constitutions to require a RFRA-like standard of review (Volokh 2013). Lupu (2015) and Volokh (2013) classify differently which states have interpreted their constitutions to require a RFRA-like standard. I return to this issue in Section 3.B, where I note that the lack of clarity regarding the legal status in these states was a reason to avoid sampling businesses from them.

[7] Connecticut, Illinois, New Mexico, and Rhode Island.

[8] Maine, Massachusetts, Minnesota, New York, Vermont, Washington, and Wisconsin.

in conservative states, such as Dallas, Indianapolis, Phoenix, and Atlanta (Lupu 2015, p. 49). To date, no local government in an AD state has enacted a local RFRA.

In summary, the regulation of the tension between marriage equality and religious liberty divides into four categories: regimes (state or local) with both AD laws and RFRAs; AD-law-only regimes; RFRA-only regimes; and regimes that have enacted none. This is the diverse legal background against which *Masterpiece Cakeshop* was decided.

B. Masterpiece Cakeshop v. Colorado Commission of Human Rights

The *Masterpiece* case presented a conflict between Jack Phillips—the owner of Masterpiece Cakeshop—and Charlie Craig and David Mullins, a same-sex couple who entered his cakeshop to inquire about a wedding cake, unaware of Phillips' beliefs. Phillips declined to make the cake citing his objection to same-sex unions.

The Colorado Civil Rights Commission found that the baker discriminated against the couple based on their sexual orientation. The Supreme Court reversed and invalidated the Commission's decision, writing that the Commission showed unconstitutional religious hostility. While the baker won the case on free exercise grounds, the majority acknowledged that "if that [religious] exception were not confined, then a long list of persons who provide goods and services for marriages and weddings might refuse to do so for gay persons, thus resulting in a community-wide stigma inconsistent with the history and dynamics of civil rights laws." (*Masterpiece*, p. 1727). For these reasons, the Court did not rule out the possibility that Colorado could enforce its AD law in similar cases in the future. More generally, the majority's opinion did not expressly solve the tensed relationship between religious liberty and sexual orientation equality

3. THE *MASTERPIECE* EXPERIMENT

A. The motivation and setting for the experiment

The primary purpose of the project is to examine the contradicting empirical assumptions regarding the effects of religious exemptions on discrimination towards same-sex couples. The present contradictions and omissions have made it impossible to assess the merits of the opposing positions and leave the debate hanging in the air.

Masterpiece Cakeshop created an opportunity to evaluate these arguments in their most pressing setting. Based on the oral arguments, I anticipated that the decision would yield an exemption of sorts.[9] As "one of the most anticipated decisions of the term" (Howe 2018), the decision was likely to draw extensive coverage and discussion in the public media (as it did), thus to potentially have an impact on public attitudes and conduct (Linos and Twist 2016).

When the decision was finally rendered on June 4th, 2018, it received broad coverage and mixed responses. National, state and local news outlets covered the decision and sought comment from local advocacy groups and politicians (Simpson 2018; Platoff 2018; McGaughy 2018). All mainstream outlets, including the New York Times, NBC News, and CNN, titled the decision a victory for the baker; they also called the decision "narrow," explaining that it did not resolve the big constitutional questions at issue (Liptak 2018; Williams 2018; Goldfeder 2018). At the same time, many conservative leaders and religious liberty advocates hailed the decision as a victory,

[9] This expectation was based on the comments of Justice Kennedy, then the Court's swing seat, who hinted that the Court thinks that there was "a significant aspect of hostility to a religion in this case", Tr. of Oral Arg. 53. This became a dominant line of questions for the conservative judges on the bench, *id.* at 53-59. Justice Kennedy also said unequivocally, "Counselor, tolerance is essential in a free society. […] It seems to me that the state in its position here has been neither tolerant nor respectful of Mr. Phillips' religious beliefs." *Id.*, at 63.

expressing less reservations about its scope.[10] Fox News Insider (2018) held a supportive interview with Phillips, who defined the decision as a "big win."[11] Leaders of the U.S. Conference of Catholic Bishops released a joint statement applauding the decision, saying that it "confirms that people of faith should not suffer discrimination on account of their deeply held religious beliefs, but instead should be respected by government officials" and emphasizing the decision's expression of pluralism and tolerance (Catholic News Agency 2018). The Family Research Council (2018) released a statement that the decision "made clear that the government has no authority to discriminate against Jack Phillips because of his religious beliefs" and that the "ruling means that Jack will remain free to live according to his beliefs whether he is at work, at home, or in his place of worship." These statements do not betray any doubt about the scope of the decision or mention the Court's recognition of the important role of AD laws in protecting LGBTQ people.

Some LGBTQ advocates and progressive commentators observed these enthusiastic responses and voiced concerns that *Masterpiece* will grant objectors a license to discriminate. GLAAD president said that "it leaves the door wide open for religious exemptions to be used against LGBTQ people" (Lang 2018). The president of LGBTQ Victory Institute further warned that "Homophobic forces will purposefully over-interpret the ruling and challenge existing non-

[10] To give the readers a better sense of the conservative and religious framing of the decision, I survey key quotes in the footnotes. Consider Kao (2018): "the decision… [exposed] a huge fallacy in the ACLU's main argument in the case… The court's clear rejection of the discrimination argument has implications for many of the other conflicts currently brewing between religious freedom and sexual orientation."; Liberty Counsel (2018) :"Though the Court focused on the explicit hostility exhibited by the Colorado Civil Rights Commission in this specific instance, this significant decision will have a wide impact regarding the clash between free speech and the LGBT agenda, including laws that add 'sexual orientation' and 'gender identity.'"

[11] *See also* Starnes (2018) ("Monday's ruling should give some comfort to Christian business owners who primarily service the wedding industry – gay rights do not necessarily trump everyone else's rights"). Other coverage by Fox News was more careful in discussing the decision, *e.g.* Mears (2018) ("The narrow ruling here focused on what the court described as anti-religious bias on the Colorado Civil Rights Commission when it ruled against baker Jack Phillips.").

discrimination laws by refusing service to LGBTQ people in even more situations" (Lang 2018). NBC's columnist Scott Lemieux (2018) wrote that the decision "presents a serious risk of undermining civil rights law in the name of religious freedom, especially given that it invites yet further suits for the court to consider."

This combination of factors—a highly anticipated decision, a court that appeared positioned to exempt the religious objector, and the massive coverage that communicated the above messages—created a favorable setting for an empirical test of the behavioral effects (or lack thereof) of religious exemptions. In a previous study, Linos and Twist found that Supreme Court decisions can increase support for controversial policies that were vindicated by the Court (e.g., Obamacare), even when the court was divided and the decision was nuanced (Linos and Twist, 2016). Similarly, three recent studies that measured the effect of same-sex marriage legalization on public attitudes documented increases in support for same-sex marriage post-*Obergefell* (Kazyak and Stange 2018); increases in the perception that social norms support same-sex marriage (Tankard and Paluck 2017); and, prior to *Obergefell*, sharper decreases in antigay bias in states that legalized same-sex marriage compared with states that did not (Ofosu et al. 2019). All of these studies were based on attitudinal surveys conducted shortly before and after court decisions or legislative acts, sometimes with an additional experimental component that randomized the framing of the decision. Additional surveys examined public attitudes about LGBTQ discrimination, irrespective of Supreme Court decisions or same-sex marriage legalization (Powell, Schnabel, and Apgar 2017). Yet none of these studies examined the impact of Supreme Court decisions on the behavior of pertinent decision-makers. In the present case, the pertinent decision-makers are wedding vendors and the question is whether they are influenced from a ruling for a vendor who denied

service to a same-sex couple.

In addition, most previous studies did not investigate whether the effect of Supreme Court decisions varies between socio-legal regimes. Yet different legal regimes can direct wedding vendors towards different behaviors and therefore differentiate their response to *Masterpiece*. For example, businesses in regimes that resemble Colorado—with AD laws and without RFRAs—might adopt their response to same-sex couples if *Masterpiece* is perceived as relaxing their AD obligations. One may expect such change to be more pronounced in overlap regimes, because the existence of a RFRA could strengthen the impression that businesses are likely to secure an exemption post-*Masterpiece*. In contrast, businesses in regimes that have never enacted AD laws have no legal basis to change their behavior. For these businesses, the law has not changed: they are as free to discriminate after *Masterpiece* as they were before the ruling.

In sum, *Masterpiece* provided a unique opportunity to study the behavioral effect of religious exemptions from antidiscrimination law, a question of which no empirical data exist to date, and which bears heavily on contemporary legal debates. In addition, the study advances the research of the effects of Supreme Court decisions in several ways: First, the study expands the examination from attitudes to behaviors, and from the general public to pertinent decision-makers. Second, the study goes deeper than previous studies in probing the relationship between the national judicial "shock" and the preexisting legal structures that could vary the effect of the decision between otherwise similar subnational regimes.

B. Research design and methods

To assess whether *Masterpiece* had an effect on sexual orientation discrimination in the

wedding industry, I combined methods from pseudo- and field-experiments. As in pseudo-experiments, I examined the behavior of wedding in businesses in two periods: before (May 8th-15th) and after (June 13th-20th) the decision (June 4th, 2018). As in field experiments, the methods of this examination aimed to control the setting of the examination and the allocation of the sexual orientation treatment between subjects (businesses) to allow for causal inference. The following subsections describe the construction of the sample, materials, and procedure of the experiment.

Sample. Sample construction began with a preliminary comparison of all states, to find those that were most comparable in their overall characteristics yet differed in their legal regime. The comparison included GDP per capita, the importance of religion for state residents, the share of Evangelicals in the state, the share of conservatives, attitudes towards homosexuals, and attitudes towards same-sex marriage. After matching demographic resemblance against legal regime variation, four states were selected for sampling: Indiana, Texas, Iowa and North Carolina. Table 1 shows that these states have roughly the same attitudinal and economic characteristics, that are either at the national average or more conservative.

TABLE 1 – CHARACTERISTICS OF SAMPLED REGIMES

Criterion	Definition	Iowa	North Carolina	Indiana	Texas	Dallas Metro, TX	Houston Metro, TX
GDP per capita ($)		59,978	54,442	55,173	61,168	--	--
Importance of Religion	Religion is Somewhat/Very Important (National average: 77%)	79%	84%	78%	86%	85%	83%
% Conservatives	(National average: 36%)	41%	40%	41%	39%	41%	38%
% Evangelicals	(National average: 25%)	28%	35%	31%	31%	38%	30%
Attitudes Towards Homosexuals	"Homosexuality should be discouraged" (National average: 31%)	36%	36%	37%	36%	35%	39%
Attitudes Towards Same-Sex Marriage	Opposing/Strongly Opposing Same-Sex Marriage	41%	45%	45%	46%	44%	51%

(National average: 39%)						
State RFRA	No	No	Yes	Yes	Yes	Yes
State/Local AD law	Yes	No	Some	Some	Yes	No

Notes: GDP per capita is calculated based on data from 2018, Q2. Sources: GDP: THE U.S. BUREAU OF ECONOMIC ANALYSIS, GROSS DOMESTIC PRODUCT BY STATE, SECOND QUARTER 2018 (2018); Population: U.S. CENSUS BUREAU, 2018 NATIONAL AND STATE POPULATION ESTIMATE (2018); All other data: PEW RESEARCH CENTER, RELIGIOUS LANDSCAPE STUDY (2014). State/Local AD Law (row 8) refers to public accommodation laws that apply to private businesses and are enacted at the state or city level.

Alongside their demographic and attitudinal similarity, the sampled states vary in how they regulate religious freedom and public accommodations. North Carolina has no RFRA and no AD law at any level of government.[12] Iowa has no RFRA (at no level of government) but has a state AD law (IOWA CODE § 216.7). Both Indiana (IND. CODE § 34-13-9) and Texas (TEX. CIV. PRAC. & REM. CODE ANN § 110.011) have state RFRAs and no state AD laws, yet some local governments within these States have AD laws. Sampling from all of these regimes produced a 2 (+/- AD) by 2 (+/- RFRA) sampling matrix (Table 2).

Two reasons were responsible for the choice of Texas and Indiana as models of the overlap category (+RFRA,+AD) and the +RFRA-AD category. As Part 2 describes, the overlap between RFRAs and AD laws has three versions: (1) states that enacted both laws; (2) states that enacted an AD law and their courts interpreted their constitution to provide a RFRA-like standard; and (3) local AD laws within RFRA states. The primary reason for choosing the third version to model the category was that the demographic and attitudinal characteristics of the four states that enacted both laws (RI, CN, NM, IL) and the states that only had RFRA, without an AD law, differed too widely than the states populating the other matrix categories. Second, not all RFRA designs raise the same tension with AD laws. RFRAs in the first overlap category are mostly narrow and not

[12] NC has enacted a specific religious exemption allowing magistrates to refuse to perform same-sex marriages, SB 2 (2015), but there is no exemption for private businesses. NC also prohibited cities or counties from passing sexual orientation AD laws, HB 2 (2016), what explains the complete absence of local AD laws in the state.

conducive for the examination of this tension, while the second category raises considerable uncertainty regarding the very existence of the tension (Volokh 2013, cf. Lupu 2015). Texas and Indiana provided an adequate demographic and attitudinal comparison to the other legal categories, as well as clarity regarding the classification of their legal regimes.

TABLE 2 – THE LEGAL REGIME MATRIX AND SAMPLED STATES

	RFRA	No RFRA
AD law	Specific jurisdictions in Indiana and Texas[13]	Iowa
No AD law	Specific jurisdictions in Indiana and Texas[14]	North Carolina

To be sure, although I invest great efforts in facilitating the comparison between regimes, I do not argue that this design is capable of identifying a causal relationship between specific regimes and behavioral outcomes. First, it is difficult to separate the legal regime from the underlying political and social climate that produced the law.[15] Second, different laws provide different behavioral guidance—as discussed above—but businesses might not be fully aware of law's dictates. Nevertheless, legal regimes are likely to matter, if not for the direct impact of law then for the underlying socio-political structures that it reflects. Had I only sampled from one regime, important variation would have been masked and misleading interpretations might have been construed. Exploring how businesses in different regimes respond to *Masterpiece* is thus

[13] At the time of the experiment, these included the following: in Indiana, Indianapolis, Fort Wayne, Evansville, Bloomington, Muncie, South Bend, Terre Haute. In Texas, Dallas, San-Antonio, Austin, El-Paso, Plano, Fort Worth.

[14] At the time of the experiment, these included the following: in Indiana, West Lafayette. In Texas: Houston, Irving, Arlington, Corpus Christi, Lubbock, Garland, Amarillo, Grand Prairie, Brownsville, McKinney, Killeen, McAllen, Waco, Denton, Round Rock, College Station.

[15] In addition, the law is determined not only based on acts of the legislature but also based on judicial decisions and administrative directives that interpret the enacted rule. I attempted to account for those—for example, by not sampling from overlap states where courts interpreted RFRAs as providing no protection against AD claims—but it is very difficult to account for all interactions between judge-made law and legislated law.

necessary, even if the results are only suggestive and causal inference is limited.

A power analysis (via G*Power) determined that a sample size of 179 businesses per legal category is needed to detect a medium-size effect (.25) with 80% power (assuming four legal regime groups and two covariates—see below). The detection of within-subject effects (sexual orientation and the effect of *Masterpiece* itself) required a considerably smaller sample. Yet due to pitfalls that could result in sample reduction (e.g., inactive businesses or email addresses; technical failures) the sampling aimed for 250 businesses per legal category.

The wedding industry includes a variety of vendors and services, such as photography, videography, flower arrangements, dresses, suits, wedding cakes, wedding planning, venues, and more. Recent cases in which businesses refused service to same-sex couples involved bakers, photographers, and florists, among others. I was particularly interested in bakers and photographers, because these businesses represent different models of involvement in the wedding: photographers typically spend many hours with the couple, take an active part in the event and are present throughout the wedding, often for 9-10 hours. Typically they also create the couple's wedding album, requiring continued relationship with the couple. In contrast, bakers typically have a more limited interaction with the couple (during tastings and the order), do not play an active role in the event and are not present in the wedding. These differences in personal involvement could bear on vendors' willingness to serve couples. Therefore the sampling focused on these two types of vendors, supplementing them with florists in one legal regime (Iowa) where not enough photographers and bakers were found.[16]

[16] Iowa contained smaller populations of bakeries and photographers compared with the other regimes (and particularly bakeries). Florists were chosen to augment the sample because of prior conflicts involving this industry

The sample was built by collecting all vendors in each legal regime that could be found on a simple Google search and published an email address as a form of communication.[17] Contacting wedding vendors via email is a very common, if not the most common method of communication of couples: There is ample guidance online on how to write an email to potential wedding vendors and multiple websites assume that email is the default or best form of communication with vendors.[18] After mapping states and cities/counties that fitted into the legal regime typology, businesses were sampled based on regime size, from large to small. Thus, the sampling gave preference to large political units (e.g., big cities) over small political units (e.g., small cities and rural counties) and ended when the designated sample size was obtained.[19] Each business included in the sample was individually checked and verified to be a relevant business (e.g., a bakery rather than a coffee shop). The final sample includes the entire population of bakeries that met the search criteria in each legal regime, and a large sample of the respective photographers' population.[20]

(e.g., *Arlene Flowers, supra* note 2) and because florists' involvement in the wedding was assumed to be intermediate: florists do not fill an active role in the event and are not present throughout the event, similar to bakers; and they do not appear to be offering shelf products, similar to photographers.

[17] The search words were "[profession] in [jurisdiction]" (e.g., "wedding photographers in Indianapolis, IN").

[18] *See, e.g.,* Forrest (2017) (assuming communication is done vie email); Currey (2019) (providing guidance on how to write emails to potential wedding vendors); Malie (2018) ("An email is usually the preferred method for inquiries as it allows the vendor to keep track of your conversation, respond in length and from a desktop, and allows them to easily attach files, reference links, and more."). Vendors that did not publish an email address typically had an online application form on their website, reducing the potential concern that the sample is biased towards technology-oriented vendors.

[19] If the search yielded more results than needed, only the first valid results were included. The rational for including top results rather than a random sample of search results was based on the Google search algorithm, which prioritizes relevant results, and also on the researchers' experience that first result pages include more relevant results than advanced pages. Top results were typically within the geographic boundaries I searched for, whereas later results were often in suburbs or other cities/counties; in addition, top results typically met the definition of the searched business, whereas later results sometimes belonged to other types of businesses (e.g., a Starbucks coffee shop that came up in a wedding bakeries search).

[20] In Iowa (+AD – RFRA), the sample also includes the entire photographer population (Iowa was the only category in which the search was not able to collect 250 businesses and exhausted all business types at 218 businesses). In North Carolina (- RFRA - AD) the sample exhausted 89% of relevant photographers. In the +RFRA -AD regime (Texas and Indiana) the sample exhausted 88% of the population. In the +RFRA +AD regime (Indiana and Texas) our sample exhausted about 50% of the relevant population (the photographer population in this regime was bigger than other regimes).

In addition to the main experiment sample, I also constructed a "control" group of 251 vendors which was only contacted after *Masterpiece.* The control group was composed of photographers and florists from three of the four legal regimes (as all businesses in Iowa and bakers in all jurisdictions were exhausted in the experimental group). The control exhausted the relevant photographers population in each regime in addition to 45 florists from each regime. As explained in more detail in the procedure, the control group was not designed to test differences between regimes or business types, but to evaluate the effect of the experimental procedure itself.

Procedure and materials. Sixteen fictitious email profiles were created for the experiment. In order to assess the baseline discrimination pattern, each business received two emails prior to *Masterpiece* from two different 'couples': a same-sex couple (1st wave) and a different-sex couple (2nd wave). The couples' sexual orientation was made evident by their names. The name of the sender, appearing in the profile information and the signature, was a generic white American male name (John, Robert, Dylan, Scott). The name of the prospective spouse appeared inside the body of the email and was a generic name for a white American male or female, depending on the couple's identity (Adam, Paul, Harry; Jessica, Ashley, Rebecca). The emails had similar properties, including similar information about the fictitious couple, the service requested from the vendor, and the prospective timing of that service,[21] and they were written in the same level of cordiality. Small, meaningless changes were inserted to diminish suspicion (including variations in wording, font size, font color, and signature style). The emails were sent one week apart, during

[21] The timing of the prospective wedding noted in the email was determined based on market norms about when approximately to contact specific vendors. While the recommended timeline somewhat varies from one wedding portal to another, the emerging norm seems to be that photographers should generally be contacted between 9-11 months in advance, and florists and cakeshops should be contacted 6-9 months in advance (some advices provide a shorter timeframe for cakeshops). Thus, emails to photographers noted a wedding date which was 11 months away and emails to florists and cakeshops noted a date which was 8 months away.

business days and hours, at about the same day of the week and time of the day, with an intentional hour lag to reduce suspicion.[22] All email versions are included in the Online Appendix.

A week after *Masterpiece*, on June 13th, all businesses were randomized to receive an email from a same-sex or a different-sex couple (3rd wave); and on the following week, each business received an email from the opposite-orientation couple (4th wave) (Overall, each business received emails from both couple types, one week apart). The emails of each wave had similar properties and were different from the two pre-*Masterpiece* emails. To increase responsiveness and further distinguish the communication from previous waves, post-*Masterpiece* emails included profile pictures and phone numbers, and alternated further the style and formatting of the emails. Each email was always sent from a profile that has not contacted that business before; altogether, each business received four different emails from four different profiles.

Businesses of the control group were contacted for the first time in the 3rd and 4th waves (after the decision was rendered), following the exact same post-*Masterpiece* procedure and during the exact same times. The control group was included to evaluate the possibility that the repeated measurement of businesses in the experiment group could have influenced their behavior, independent from the effect of the decision or couple's identity. More specifically, the goal of the control group was to assess whether the effects of *Masterpiece* were similar among businesses that were contacted both before and after *Masterpiece* and businesses that were freshly-contacted only after *Masterpiece*. This comparison provided an independent reference for response rate and attrition rate, which allowed for an additional robustness check.

[22] In each wave, some subjects received the email 24 or 48 hours after the main group, due to logistic issues. All emails were sent during business days and hours.

In order to reduce suspicion and fatigue, the research team answered each responding business manually once, soon after the response was received, and before the next wave of emails. The answers were personal and varied based on each business' response. Typically, the responses requested more time to think or mentioned a reason for not continuing the correspondence which was unrelated to the details of the offer. Fifty-nine vendors had to be excluded from the sample because of email communication failures (typically, not receiving one of the four emails), and 15 were omitted due to explicit or potential suspicion.[23] The final sample, after exclusions, remained significantly larger than required to detect the minimal effect, based on the power analysis:

TABLE 3 – THE FINAL SAMPLE OF BUSINESSES IN THE EXPERIMENT

	RFRA	No RFRA
AD	N = 212 Photographers: 125 Bakers: 87	N = 210 Photographers: 93 Bakers: 35; Florists: 82
No AD	N = 244 Photographers: 179 Bakers: 65	N = 238 Photographers: 155 Bakers: 83

For any given inquiry, I measure the response it elicits from the business based on the email communication. Two RAs coded the entire dataset of emails under the close supervision of the PI. The research team conducted numerous meetings throughout the coding process to discuss the coding method, resolve open issues, and fine-tune the coding scales.[24] The main outcome of interest in all analyses is whether businesses agreed to provide service to the couple.[25]

Between the 3rd and 4th email waves a phone survey was conducted with a random sample of

[23] Online Appendix, Section OA2, details reasons for exclusions and the number of vendors excluded.

[24] Online Appendix, Section OA5, provides further information regarding the coding process.

[25] I had two measures for this outcome, binary and nuanced. The scale for Nuanced Response was: 1 = positive response, 0.5 = cooperative response, e.g., a vendor that asks for more information on the date or the location, 0 = no response, -0.5 = a negative response that includes a referral to other providers/services', -1 = negative response. Binary Response coded all responses > 0 as 1 (positive) and all responses <= 0 as 0 (negative).

wedding vendors to gain insight on non-response patterns observed in waves 1 and 2 (See below). The appendix describes the phone survey's sample, procedure, and results.

C. Strengths and Weaknesses of the Experiment

The experimental design has multiple methodological strengths. First, it combines two of the most powerful methods for causal inference—pseudo-experiments and field experiments—to enable the study of an actual, concrete event—the *Masterpiece* decision—in a controlled setting. Second, sending carefully designed materials of fictitious individuals instead of real auditors creates a controlled setting for the study and removes inadvertent auditor biases (Bertrand and Mullainathan 2003). Third, the outcome measure—agreement to provide services to the couple—is less crude than, for example, callbacks in employment experiments (Bertrand and Mullainathan 2003). This is because the argument about discrimination in wedding services relates to the specific stage of the transaction that is studied here: the initial inquiry about the service.

Alongside these strengths, the experiment also has limitations. First, similar to Bertrand and Mullainathan (2003), I study asynchronous communication rather than face to face or phone communication. There are good reasons for that: Using emails enables the creation of a highly controlled experimental setting, including with respect to the couple's identities, how they represent themselves to businesses, the exact content of the inquiry, and the timing of the inquiry. All of these are very difficult to achieve in studies that employ real testers (audit studies). Although testers/auditors can be trained to behave similarly, it is impossible to erase the numerous differences between real people, or control for nuances in tone and facial expressions that can disclose the auditors' attitudes or that their search for a job/service is ingenuine (Bertrand and Mullainathan, 2003). In addition, while email inquiries do not capture the entire variation in how

couples interact with vendors, emails are one of the most common methods of communication between couples and vendors, especially in the inquiry phase. To the extent that the process of negotiating with vendors has even moderate friction, one would expect that reduced positive responses to emails would ultimately translate into less market opportunities for same-sex couples. However, how the results translate to additional methods of communication remains an open question. This can be a topic for a future study.

An additional limitation, that flows directly from the method of a pseudo and controlled experiment, is that I examine the effect of *Masterpiece* in a relatively short time span: several weeks after the ruling. While collecting more observations would have been desirable, it was not possible to continue isolating the effect of the decision from intervening political developments. Shortly after the decision, legislatures in Texas, Iowa, and North Carolina have proposed, revived and even passed legislation pertaining to religious liberty and LGBTQ rights (Rodriguez 2019; Platoff 2019a; Platoff 2019b; Fitzsimons 2019). In this dynamic landscape, the conduct of businesses can no longer be linked to *Masterpiece*. Future cases may provide opportunities to examine additional effects of religious exemptions on society.

TABLE 4: OVERALL RESPONSE RATES IN EACH WAVE

Wave	Overall Response Rate
W1	70.8
W2	58.7
W3	63.4
W4	61.9

Finally, and this is an issue pervasive in studies that repeatedly measure the same respondents over time (Kalton, 2009; Gerber and Green 2012, pp. 236-238), I encountered a large attrition of businesses in the second wave of inquiries before *Masterpiece* (See Table 4). This pattern hindered

the ability to detect discrimination in the pre-*Masterpiece* period, as the first wave of emails was from same-sex couples and the second wave of emails was from opposite-sex couples. While the causes for this attrition are not entirely clear (this is common to studies that encounter attrition, Fitzgerals et. al 1998), a random phone survey suggested that businesses that provided no response to the second wave of emails were generally less responsive than other businesses (also over the phone), rather than suspicious or email fatigued.[26] To minimize the impact of attrition on the robustness of the design, in the following waves I randomized couples' identity within each wave. In addition, as described in the procedure, the following waves were designed to increase responsiveness by altering the style and formatting of the emails and the couples' profiles.[27] This effort succeeded in increasing responsiveness to wave 3 and in reducing attrition between waves 3 and 4. Nevertheless, I concede that the attrition of businesses from wave 2 prevents the evaluation of the existence and extent of sexual orientation discrimination before *Masterpiece*.[28] To overcome this pitfall and evaluate the effect of *Masterpiece* on the existence and extent of discrimination *after* the decision, I developed several strategies of analysis which I present next.

4. RESULTS

This Section is organized as follows: it begins with the main analysis, which focuses on businesses that agreed to serve same-sex couples before *Masterpiece* and examines their behavior post-*Masterpiece*. The second analysis examines within-business changes of behavior across all

[26] Online Appendix, Section OA3, reports that vendors who did not respond to the 2nd wave email were also less likely to answer the phone than vendors who replied to that email (36% vs. 52%, respectively). In addition, no 'phone favoritism' was found among email non-responders, *id*.

[27] Among these alterations, each profile was allocated a unique phone number, including a personalized outgoing message (identical in content).

[28] It is possible to infer that prior to *Masterpiece*, opposite-sex couples were disfavored relative to same-sex couples (reversed discrimination), but this inference seems tenuous. To the extent it is true, the magnitude of the *Masterpiece* effect is much larger than estimated below. Section OA4.1 in the Online Appendix reports these results.

businesses over time. I then move to examining differences between legal jurisdictions and between religious environments. Robustness checks and follow up studies can be found in the Online Appendix and are referenced along the way.

A. Has Masterpiece Increased Discrimination Towards Same-Sex Couples?

To answer this question, I evaluate the impact of *Masterpiece* on businesses that agreed to serve same-sex couples before the decision (N=575 businesses * 2 post-*Masterpiece* observations per business, resulting in 1150 observations). The assumption underlying this analysis is that businesses who responded favorably to same-sex couples before *Masterpiece* were equal treatment businesses (for the very least, they were "gay-friendly" businesses). Examining their behavior *after* the decision can provide an answer as to whether *Masterpiece* had a negative effect on the willingness of businesses to provide service to same-sex couples.

Some readers might have expected that a diff-in-diff strategy would be applied to answer the question. But the problem with the diff-in-diff approach is that the attrition between the first and second waves (both of which were pre-*Masterpiece*) meant that a diff-in-diff would be subject to criticism as not having comparable opposite-sex-couple-treated observations. Focusing on businesses who were gay-friendly in the pre-*Masterpiece* period circumvents the need to rely on data from the second wave of emails before *Masterpiece*, where significant attrition occurred. This is possible because all emails from same-sex couples were sent in the first wave of emails and therefore data on responsiveness to same-sex couples were not affected from the subsequent attrition.[29] Because assignment to wave in the post-period was randomized (unlike in the pre-

[29] Ideally, had attrition not occurred, data from all waves should have been used to simultaneously assess both the existence of discrimination pre-*Masterpiece* and the effect of *Masterpiece* on preexisting discrimination. But because the data from wave 2 suffers from significant attrition, it is not possible to evaluate pre-*Masterpiece* discrimination

period), there is no concern about attrition creating a confound if one conditions on the pre-period behavior and uses post-period outcomes to measure the effect of the decision.

Second, while the sample is smaller than the original sample (N= 906), it is still large enough to detect the effect in question.[30] The self-selection of businesses into this sample is not a concern, since these are exactly the businesses that require our focus. Businesses that discriminated against same-sex couples both before and after *Masterpiece* would not influence the results. In contrast, businesses that shifted from positive response to discrimination are precisely the object of the inquiry. Because businesses were randomized post-*Masterpiece* to receive an inquiry from a same-sex or an opposite-sex couple (and the vice versa in the following wave, such that each business was contacted by both couples post-*Masterpiece*), I can estimate the effect of *Masterpiece* precisely, using both within and between businesses data.

TABLE 5 - AVERAGE RESPONSE RATES OF LGBT-SERVING BUSINESSES AFTER *MASTERPIECE*, BY SEXUAL ORIENTATION OF COUPLES (NUANCED RESPONSE)

Sample	Couple	Negative Responses		No Response	Positive Responses		Total PR	Percent Difference in PR	Total RR	Obs.
		Neg.	*Neg. w/ Referral*		*Coop.*	*Pos.*		*(p-value)*		
All Inquiries	Opposite-sex	2.78	2.08	19.62	2.26	73.26	75.52	9.20	80.38	576
	Same-sex	4.17	1.74	27.78	1.74	64.58	66.32	(0.0006)	72.22	576
Wave 3	Opposite-sex	1.39	2.43	19.44	2.43	74.65	77.08	9.38	80.56	289
	Same-sex	3.47	1.74	26.74	2.08	65.63	67.71	(0.011)	73.26	287
Wave 4	Opposite-sex	4.17	1.74	19.79	2.08	71.88	73.96	9.03	80.21	287
	Same-sex	4.86	1.74	28.82	1.39	63.54	64.93	(0.018)	71.18	289

patterns. At the same time, it is possible to answer the second question—what the effect of Masterpiece is—by relying exclusively on the first wave of emails, as described in the text.

[30] As the effect size exceeded the conservative assumptions of the power analysis, it was detectable also in analyses that relied on smaller samples. This is also true for the robustness analysis reported below.

Notes: The table reports the response rates, by type of response, for opposite-sex couples and same-sex couples, for businesses that prior to Masterpiece provided a positive response to same-sex couples ('gay-friendly'). Response types include: Negative response (column 3), negative response moderated by a referral of the couple to a different vendor (column 4), no response (column 5), cooperative response (column 6), and positive response (column 7). The table also reports total of positive responses per couple type (column 8), as well as the difference in positive responses between couple types (column 9). Column 9 also reports the *p*-value for a test of proportions testing the null hypothesis that the positive response rates are equal across sexual orientation groups. Finally, the table reports the total response rate, positive and negative, per couple type (column 10) and the total number of observations (column 11).

Table 5 tabulates average response rates by the sexual orientation of the couple. Row 1 and 2 present the results for all inquiries collected post-*Masterpiece* by sexual orientation. Inquiries from a same-sex couple have a 66.3 percent chance of receiving a positive response. Equivalent inquiries from an opposite-sex couple have a 75.5 percent chance of being answered positively. This represents a difference of 9.2 pp, or 14 percent, that can be solely attributed to the names manipulation. Column 9 shows that this difference is highly statistically significant.[31]

Rows 3-4 and 4-5 present the same results for each wave of inquiries sent post-Masterpiece separately. On the first week after *Masterpiece,* 67.7% of the businesses randomly contacted by same-sex couples responded positively, as compared with 77% who responded positively to opposite-sex couples. This represents a difference in positive response rate of 9.3 pp, or 14 percent. On the second week after *Masterpiece,* the randomization flipped such that each business received an email from the counter-orientation couple. Of the businesses now contacted by opposite-sex couples, ~74% responded favorably, as compared with ~65% who responded favorably to opposite-sex couples. This represents a difference in positive response rate of 9 pp, or 14 percent. These differences are significant in both waves.

Note that this pattern indicates both between-subject differences (in each week, between the

[31] These statistical tests assume independence of responses. However, as tables OA4.1-3 in the Online Appendix show, the results remain significant when the analysis assumes that the responses are correlated at the business level.

random groups) and within-subject differences (across weeks, within each group). Weekly attrition cannot explain the within-subject pattern, as the rate of response went *up* in in the group that received the first message from a same-sex couple and the second message from an opposite-sex couple. In fact, as column 10, rows 3-5, show, opposite-sex couples experienced no reduction in positive responses from the first to the second wave. Only same-sex couples experienced such reduction. Yet, as Table OA4.1 in the Appendix shows, the effect of wave was not significant.

How do businesses communicate negative responses to couples? As Table 5, Column 5 indicates, the most common form of declining service is simply no response. This result is anticipated, as writing an negative response is both time costly and awkward, and the easiest way is to ignore the inquiry (Dovidio and Gaertner 2004; Bertand and Mullainathan 2003). While it is possible that failures to respond result from not receiving an email, or forgetting to respond to it, we would expect such errors to distribute randomly, and therefore equally, across couple types. This is not the case. Opposite-sex couples had a 19.6 percent chance of not receiving a response to their inquiry. Same-sex couples had a 27.8 percent of not receiving a response. That is, the chance of same-sex couples to not receive a response was 42 percent higher ($Z=3.26$, $p=.001$).

The additional checks that are reported below and in the Online Appendix indicate that the negative effect of *Masterpiece* on the willingness to provide service to same-sex couples was robust across analyses. I find it in the entire sample of businesses in the experiment (N=906): comparing the rate of positive responses to same-sex couples before and after *Masterpiece* yields a drop of 14.4 pp, or about 23 percent. Observing only the responses after *Masterpiece* in the entire sample, the rate of positive responses for same-sex couples was lower than the respective rate for opposite-sex couples in 8.4 pp on average (Section OA4.2 in the Online Appendix); We find the

same effect in the control group, where the gap between couple types was 9.5 pp (Section OA4.3 in the Online Appendix). We also find this effect among the particularly keen group of businesses that responded positively to both couples before *Masterpiece*. While these businesses remain more responsive than any other group of businesses, they too differentiate significantly between same-sex and opposite-sex couples after *Masterpiece* (~7 pp difference, Section OA4.4 in the Online Appendix). The summary of these results is presented in Figure 1, that shows that all business cohorts respond to *Masterpiece* with unfavorable treatment of same-sex couples, notwithstanding different baselines of positive response rates that characterize each cohort in separate.

In all of these analyses, I find that the *Masterpiece* effect is stable and robust to the inclusion of all experimental covariates, including the legal regime, the type of business, and the wave of inquiry. The effect is equally strong in urban areas, which are often assumed to be particularly inclusive of same-sex couples, and does not vary with political conservativeness. However, Sub-Section D finds that the effect varies with the religiosity of the environment, such that businesses in areas dense with religious congregations, and particularly Evangelical congregations, are more likely to adopt their behavior post-*Masterpiece*.

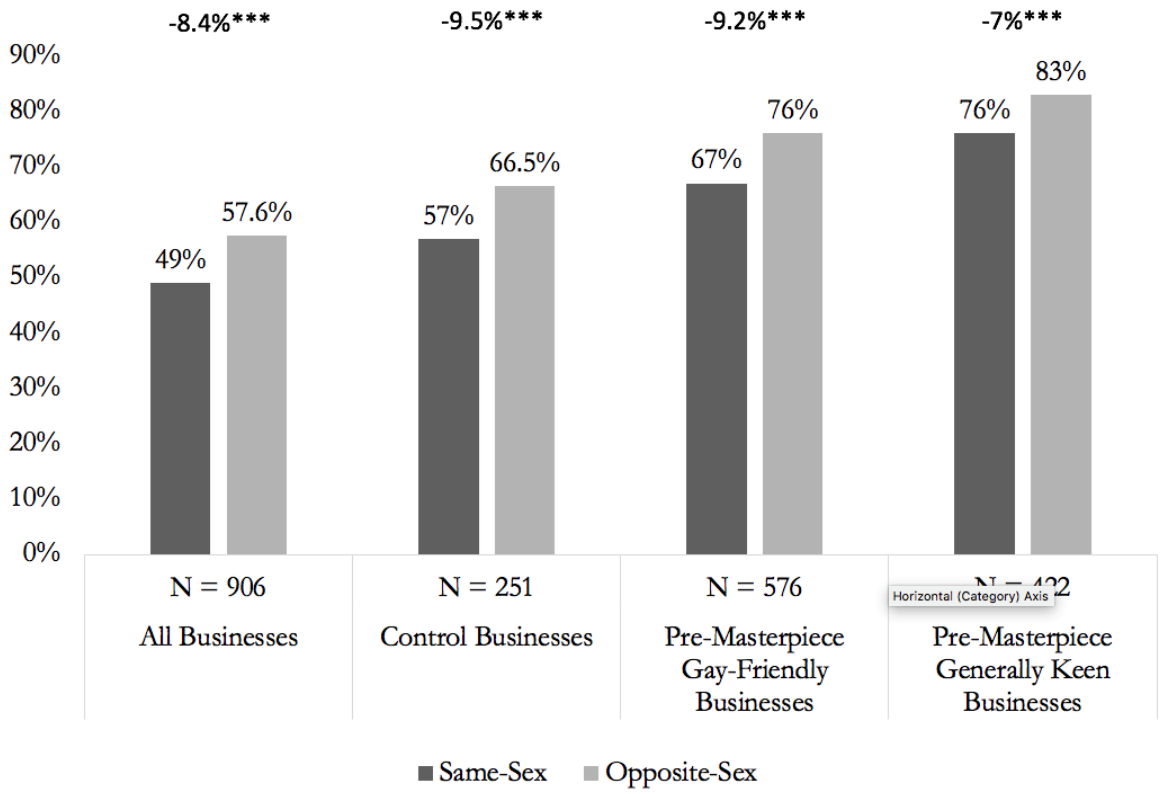

Figure 1. Agreement to serve same-sex and opposite-sex couples after *Masterpiece*

How substantial are these effects? Take the average 9% gap in the willingness to serve same-sex and opposite-sex couples that was documented in most analyses. Now consider the typical couple, that contracts with about 10 vendors in the process of organizing the wedding, including photographers, bakers, florists, videographers, venues, DJs, bridal/groom salons, calligraphers, jewelers, wedding planners, and more.[32] A conservative estimate of the number of inquiries would be one per each business category, amounting to ten in total. A more liberal (some might say more representative) estimate assumes that each couple inquires with one or two potential vendors in each category, maybe more, amounting to at least 15-20 encounters. As each vendor-couple

[32] Photographers were generally less responsive (to all couples) than other businesses, but the negative effect of sexual orientation was robust across business types.

interaction presents an independent risk of incurring discrimination,[33] the aggregate risk that same-sex couples would encounter discrimination at least once in their interactions post-*Masterpiece* is a function of the average risk posed by each vendor and the overall number of interactions. This risk ranges from 61% for ten interactions to 85% for twenty interactions,[34] and can go higher (or lower) the more (less) vendors a couple encounters.

B. Within-Business Transitions

Thus far we studied the effect of *Masterpiece* on businesses that provided favorable responses to same-sex couples prior to the ruling—that is, holding constant the pre-*Masterpiece* behavior, I asked how these businesses respond to random inquiries from same-sex and opposite-sex couples after *Masterpiece*. A complementary approach is to study within-business transitions throughout the duration of the experiment. As each business received four inquiries, two before and two after the decision, one from each couple type in each period, it is possible to study changes not only across businesses but also within businesses over time. While this analysis is compromised by the attrition that occurred in the second wave of inquiries before *Masterpiece*, zooming in on within business transitions alleviates this issue to some extent, as I will show below.

Table 6 tabulates the transitions from no/negative response pre-*Masterpiece* to positive

[33] Clearly, independent vendors in one area could be different than independent vendors in another area, as areas differ in their levels of discrimination. In that sense, the risk posed by each vendor is not entirely independent from the risks posed by neighboring vendors. The *Masterpiece* effect was robust to county-level conservativeness and city size but varied with county-level religious density. On some aspects, then, the assumption of independence holds on average, and on other aspects the risk may vary with the environment. In any event, cases of revealed non-independence were rare and were removed from the sample (Section OA2 in the Online Appendix).

[34] In probabilistic terms, the question is: what is the probability that at least one of the vendors will discriminate against the couple, given X vendors and that the average vendor poses a 9% discrimination risk? To answer the question, one needs to calculate the odds that *all* X vendors do *not* discriminate (91% per vendor) and subtract that from 1. P(at least one vendor discriminates) = $1-0.91^X$. This probability is 0.61 for X=10 vendors, 0.76 for X=15 vendors, 0.85 for X=20 vendors, and so on.

response post-*Masterpiece*, and vice versa, for same-sex and opposite-sex couples. The table indicates that transitions occur in all directions, but overall, they are significantly biased in favor of opposite-sex couples and against same-sex couples.[35] Businesses were three times more likely to transition from agreeing to declining service to same-sex couples after *Masterpiece* than transitioning in the opposite direction (row 2, column 4). In contrast, businesses were 42 percent more likely to transition in favor of providing service to opposite-sex couples after *Masterpiece* (row 1, column 4). Studying the same results at the level of the couples indicates that opposite-sex couples were more than twice as likely than same-sex couples to experience a *positive transition*, such that a previously declining business would agree to serve them post-*Masterpiece* (Column 1). In contrast, same-sex couples were twice as likely to experience a negative transition, such that a previously willing business would decline to provide service post-*Masterpiece* (Column 2). Notably, the estimates in first cell in Table 5 (row 1, column 1) should have probably been lower, because the general attrition that followed the pre-*Masterpiece* inquiry from opposite-sex couples might have yielded unintended declines of service – ultimately resulting in more positive transitions post-*Masterpiece*. The other cells, however, are not influenced from the attrition issue. In particular, the comparison of the transitions in column 2 and row 2, both showing that same-sex couples are very likely to experience a negative transition after *Masterpiece*, are unaffected.

TABLE 6 – WITHIN-BUSINESS TRANSITIONS BEFORE AND AFTER *MASTERPIECE* BY SEXUAL ORIENTATION OF COUPLES

	Percent of Businesses Transitioning From:		Percent Difference	
	Declining to Agreeing	Agreeing to Declining	(p-value)	Ratio

[35] The occurrence of transitions in all directions is not surprising in and of itself. For example, Bertrand and Mullainathan (2003) found that alongside equal treatment employers (in that study, 88% of all employers) and white-favoring employers (8.4%), there 3.5% of the employers favored black applicants. Transitions in all directions are also expected because of communication failures. Note, however, that omissions to respond were not randomly distributed across couple types, as the p-values of the comparisons in Table 5 indicate.

Opposite-Sex Couples	15.3% [139]	10.8% [98]	4.5% (0.004)	1.42%
Same-Sex Couples	7.1% [64]	21.4% [194]	-14.3% (0.0000)	0.33%
Percent Difference (p-value)	8.2% (0.0000)	-10.6% (0.0000)		
OS/SS Ratio	2.16%	0.5%		

Notes: The table reports the transition rates within-businesses from declining service (negative or no response) pre-*Masterpiece* to agreeing to provide service (positive response) post-*Masterpiece* (column 1), and vice versa (column 2), for opposite-sex couples (row 1) and same-sex couples (row 2). In brackets in each cell is the number of businesses in that cell. Column 3 reports the percent difference between columns 1 and 2, as well as the p-value of the difference, and Column 4 reports the ratio between Columns 1 and 2 (for each couple type, the ratio of positive/negative transitions). Row 3 reports the percent difference and p-value of the difference for rows 1 and 2, and Row 4 reports the ratio between Rows 1 and 2 (for each transition type, the ratio of opposite-sex vs. same-sex couples experiencing the transition).

These findings complement the main analysis by indicating that the reduction in willingness to provide services to same-sex couples that is evident across businesses post-*Masterpiece* is also evident within-businesses post-*Masterpiece*. This reduction is not offset by businesses that prior to the decision did not serve same-sex couples and transitioned to serving them after the decision (this is also evident from the analysis of the full dataset in Section OA4.2 of the Online Appendix). While transitions from a positive to a negative response occur in all directions, same-sex couples encounter the largest percent of negative directions and the smallest percent of positive transitions.

C. Does the Masterpiece Effect Vary Between Legal Regimes?

The results so far demonstrate a substantial reduction in businesses' willingness to provide service to same-sex couples, as compared with opposite-sex couples, after the *Masterpiece* decision. Next, we would like to learn more about the factors that may influence this gap. More specifically, this Section asks how this effect displays in different socio-legal regimes. Because of space limitations, the results are summarized in Figure 2. Briefly, I find that *Masterpiece* had a highly statistically significant negative effect in all regimes, *except* for regimes that enacted both

an antidiscrimination law and a religious freedom law. The full analysis is reported in Section OA4.5 of the Online Appendix. I return to these results in the discussion.

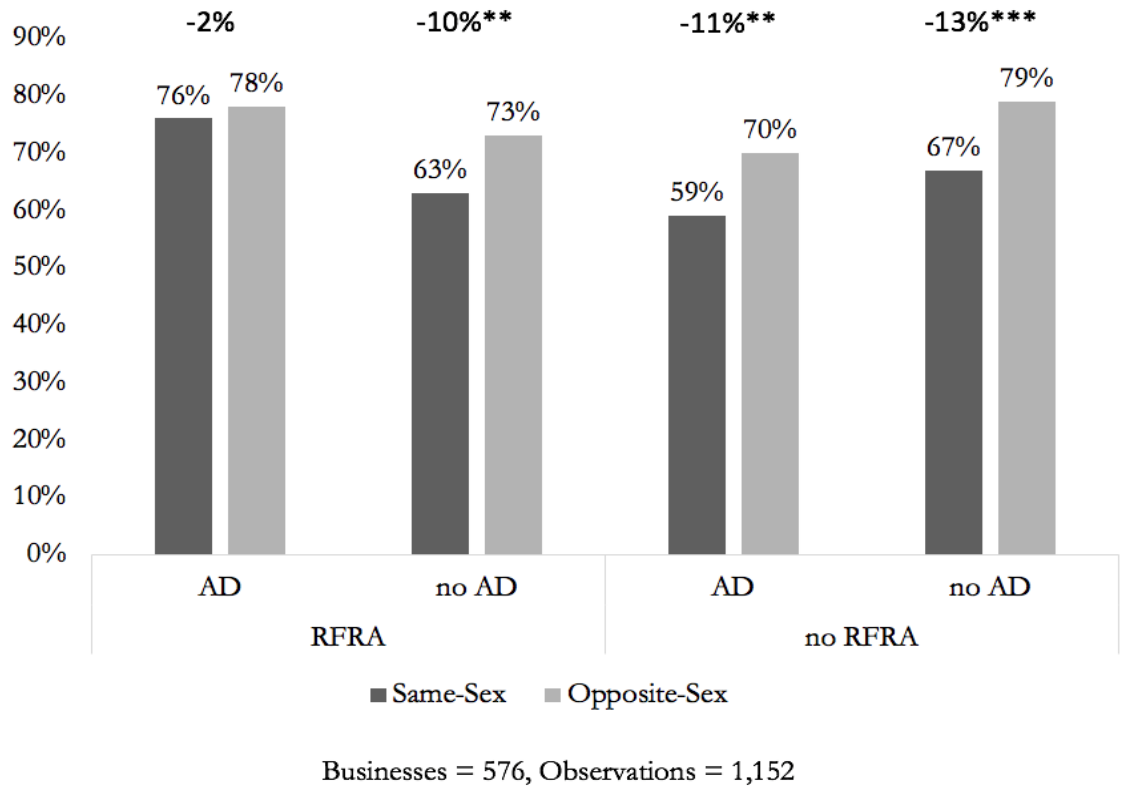

Figure 2. The effect of Masterpiece on previously gay-friendly businesses, by legal regime

D. Is the Masterpiece Effect More Pronounced in Religious Environments?

Finally, what role does religion play in business behavior? Given that the decision involves a religious exemption and received considerable attention in religious media, one may expect that businesses operating in more religious environments will be more sensitive to *Masterpiece*, and as a result, the effect will be more pronounced in these environments. This hypothesis is particularly plausible with respect to Evangelical areas, as Evangelicals have been involved in a large number of wedding conflicts and are the denomination with the lowest rates of support in same-sex

marriage (Pew Research Center 2014). Although individual-level evidence on the religiosity of the businesses is unavailable in this study, I can examine the impact of religious environment by observing the density of Evangelical congregations in the county where businesses are located.

I explored this hypothesis using public data on county-level density of religious and particularly Evangelical congregations from the U.S. Religion Census: Religious Congregations and Membership Study, 2010 (County File), available in the Association of Religion Data Archives (ARDA) (Grammich et. al 2010). The data include county-level counts of the number of congregations and adherents for 236 religious groups, as well as county population. As the number of adherents appeared less reliable than the number of congregations,[36] I computed the congregations/population ratio (religious density), for all religious congregations and for Evangelical congregations in particular. I then examine whether religious environment, as proxied by these measures, influences previously gay-friendly businesses after *Masterpiece*.

[36] As Grammich et. al (2010) explain, only about 36% of the 236 groups reported data on their adherents and in 31 counties the number of reported adherents exceeded the total population in 2010. While groups can err in counting heads or overstate their actual membership, these biases are less likely in counting congregations and are more likely to be detected by ARDA data checkers.

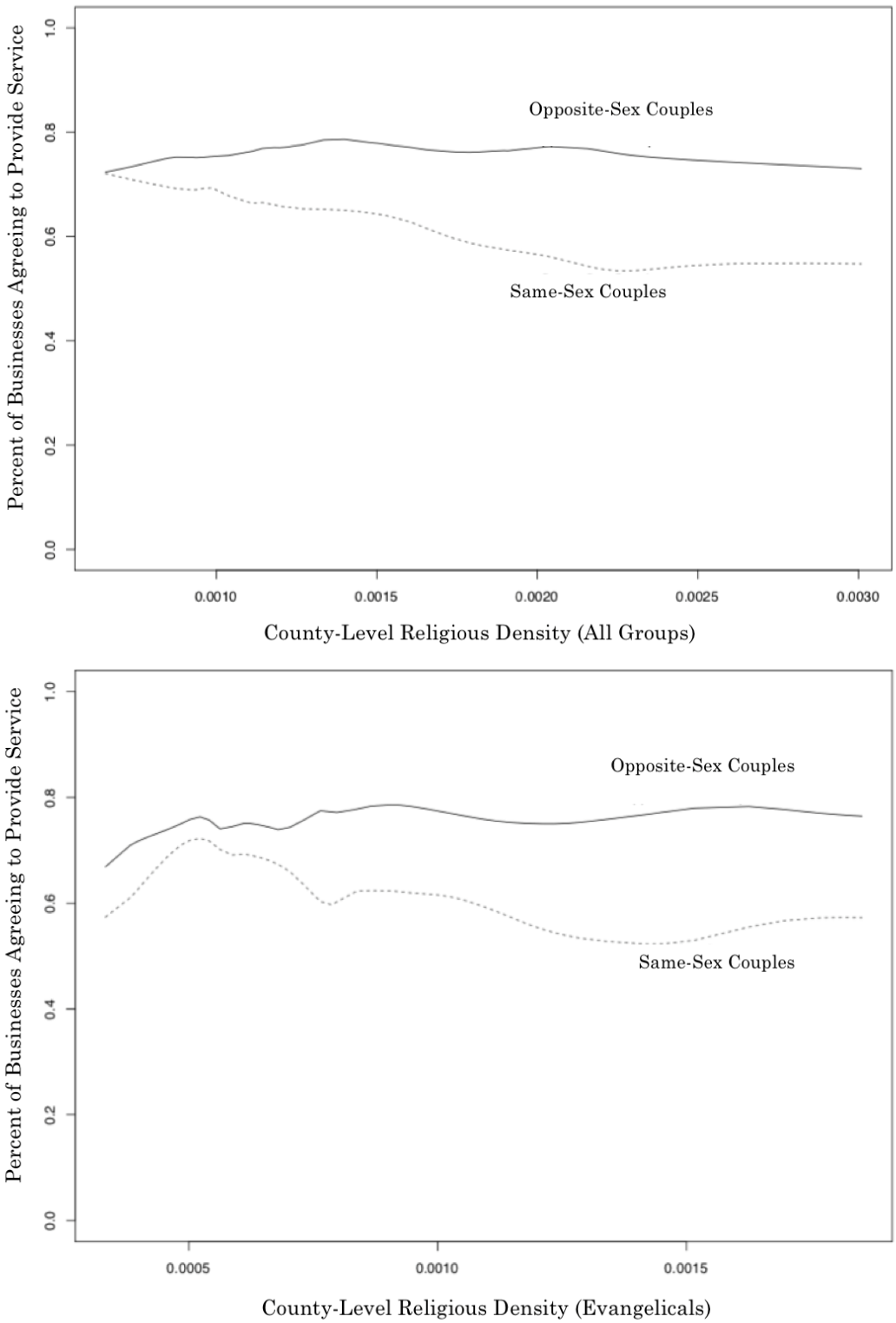

Figure 3. The effect of Masterpiece on previously gay-friendly businesses, by religious environment (top panel - all denominations; bottom panel – Evangelicals).

Figure 3 plot the results. The top panel shows that the gap in the agreement to serve same-sex and opposite-sex couples varies with the religiosity of the environment of the businesses. All of the businesses in this analysis agreed to serve same-sex couples before *Masterpiece*. After *Masterpiece*, however, businesses in religiously dense areas show a large gap between same- and opposite-sex couples. In contrast, businesses in areas with few congregations do not distinguish between same-sex and opposite-sex couples. Plotting the results against the density of Evangelical congregations provides very similar results, as the bottom panel of Figure 3 shows. The data for areas with very few congregations is somewhat noisy (only 32 businesses are located in counties where Evangelical density is 0.0004 or below), yet the general trend is the same: the sexual orientation gap widens with Evangelical density. Notably, the percent of businesses agreeing to provide service to opposite-sex couples is fairly stable across high- and low-religious/Evangelical density areas. The fluctuation occurs mostly with respect to same-sex couples.

Section OA4.6 in the Online Appendix includes the results of the regression analyses that account for religious and Evangelical density.[37] Both are statistically significant, and with the interaction term between sexual orientation and religious/Evangelical density in the model, the effect of sexual orientation is no longer significant. In other words, the negative effect of *Masterpiece* on the agreement to provide service to same-sex couples is significantly concentrated in more religious environments. To get a concrete appreciation of the magnitude of this result, I compared businesses in high vs. low Evangelical density environments (top 25% (N=141) v.

[37] One may ask how religious environment influenced behavior before *Masterpiece*. It is not possible to examine this question in full because of the Wave 2 attrition, that affected businesses across different levels of Evangelical density. However, a comparison of each pre-*Masterpiece* wave in separate shows that businesses in highly- and slightly-Evangelical environments did not significantly differ in their agreement to serve opposite-sex (p=.31) and same-sex couples (p= .71) before *Masterpiece*.

bottom 25% (N=109)). In highly Evangelical environments, previously gay-friendly businesses develop a whopping 20.5 pp gap between couples (78 pp vs. 57.5 pp, Z=3.69, p = .0002), whereas in slightly Evangelical environments, the gap is 2.7 pp (70.6 pp vs. 67.9 pp, n.s.). Observing high vs. low general religious density areas yields the same results.[38] These results indicate that businesses in more religious areas updated their behavior after *Masterpiece* significantly more than businesses in less religious areas.

5. CONCLUSIONS AND IMPLICATIONS

Combining methods from pseudo-experiments and field-experiments, this study finds a robust reduction in the willingness to serve same-sex couples after the Supreme Court *Masterpiece* decision, as compared with opposite-sex couples. The negative effect of *Masterpiece* on same-sex couples is evident in the population of businesses that prior to *Masterpiece* were willing to provide service to same-sex couples, as well as in the entire sample of businesses drawn from four different legal regimes in different US States. We see the causal effect of *Masterpiece* both within businesses, over time, and between businesses randomly contacted by same-sex or opposite-sex couples after the decision was rendered. The negative effect of *Masterpiece* is not an artifact of the experiment, as it is identically found in the control group.

What explains the effect of the *Masterpiece* decision on wedding vendors? The first aspect of this question relates to the *cognitive mechanisms* that translated *Masterpiece* to behavioral change. First, *Masterpiece* may have been interpreted as relieving a previously-anticipated penalty for discrimination or reducing the likelihood of enforcement (Becker 1968). Yet this explanation

[38] A 17.2 pp vs. 3.6 pp gap, respectively (High-density N=144; Low-density N=110; Z=3.133, p = .0017.

appears less plausible given the design of the experiment and the observed data. The experiment eliminated the risk of getting caught by design, as emails allowed vendors –both before and after *Masterpiece* – to entirely avoid detection, by simply ignoring the sender. In addition, vendors changed their behavior even in regimes that do not prohibit sexual orientation discrimination (Figure 2). A more likely possibility is an expressive effect (McAdams 2000; Sunstein 1996), i.e. that the decision led vendors' to perceive the social norm as more permissive of service refusal to same-sex couples. This explanation is supported by Tankard and Paluck (2017) and Kazyak and Stange (2018) who found that the *Obergefell* decision shaped individuals' perceptions of the social norm regarding same-sex marriage. Whereas the *Obergefell* court emphasized marriage equality, the *Masterpiece* court stressed the need to tolerate religious objection to marriage equality. The decisions may have had similar effects on social norm perceptions, only in opposite directions.

The second explanation for the effect could be the *social environment*, and particularly its religiosity, as businesses in religiously-dense areas updated their behavior substantially more than businesses in less religious areas. This may be explained by a greater concentration of religious vendors in these areas, or greater diffusion of conservative frames of the decision that may have influenced also non-religious vendors. The precise mechanism by which religion translated *Masterpiece* to negative consequences should be examined in future studies.

The findings from the *Masterpiece* experiment have several important theoretical implications. First, the experiment indicates that the Court can extend its influence beyond shaping public attitudes to shaping behavior itself. Second, the novel examination of the interaction between national and subnational legal structures highlights that the Court's effects can vary between legal regimes, sometimes in unexpected ways. In the present setting, *Masterpiece* had an effect in

regimes that were not supposed to be influenced from the decision (no AD regimes) and had no effect in regimes where change were expected (AD+RFRA regimes). These results underscore the need to account for subnational variation in future studies. Third, the study sheds new light on the argument that courts are ineffective at spurring social change (Rosenberg 2008), by highlighting the risk that the court will be effective, paradoxically, in spurring *inadvertent* social change. Rosenberg argued that courts are uncapable of shaping public opinion but they may produce backlash by mobilizing opponents to block further rulings or reverse existing ones by means of legislation (Rosenberg 2008, pp. 362-370; 418-19). This study adds another risk to this list: that unsuccessful litigation will produce the opposite social consequences, independent from any political mobilization, by influencing market players. The *Masterpiece* Court clearly worried that its decision might increase discrimination against same-sex couples, a concern which may have contributed to the narrow ruling. Were the consequences different had the Court provided a bright line rule rather than "something for everyone"? This question could be explored in future studies.

Last but not least, the present study brings crucial and heretofore unobserved data to bear on a central normative question in constitutional law, one that is essential to the resolution of conflicts between equality and freedom. First, the results discredit the argument that religious exemptions will not expand discrimination. Instead, what the *Masterpiece* experiment shows is that even a narrow exemption can have a significant and robust impact on a market and its customers. These findings advance the debate on the impact of religious exemptions: now that data are available, more nuanced analyses can be performed: We can estimate the actual, aggregate risk of discrimination (which, as I showed above, is likely to be very high); and we can direct research efforts to the factors that are found to aggravate or attenuate the risk (including legal regime and

religious environment), and divest efforts from arguments that have been contradicted in fact (including that discrimination is not an issue in urban or progressive areas). These findings provide concrete guidance for future research.

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EXHIBIT D

Online Appendix

Table of Contents

Section OA1 – Email Versions

1st Wave: Same-Sex

Bakeries:

Title: Wedding Cake Inquiry

Hi,

My name is John and my partner Adam and I are engaged to be married on January 2019. We are specifically looking for a traditional tiered cake with a personalized topper, and from what we saw on your website, we think you may just be the perfect one to help us realize that dream.

Getting down to business, what is the price range we should expect, and are there any additional services you supply, that may be of interest to us?

In addition, what kind of shelf product alternatives do you make, how much do you think that will lower the overall expenses?

Thank you for your time, I look forward to hearing from you.

Best Regards,

John ____

Photographers:

Title: Wedding Photography Inquiry

Hi,

My name is Robert and my partner Adam and I are engaged to be married in April, 2019. We're looking for a photographer, and we really loved your work, so we were wondering if you are generally available during that time to help us fulfill our wish of a dream wedding.

Would it be possible, before we proceed, to see a sample wedding album you made? We would also like to know the price range of a "standard package" for a weekend wedding and what said package would cover?

Furthermore, are there any additional services you provide, that may be of interest to us?

Thank you for your time, I am really looking forward to hearing from you.

Best Regards,

Robert ____

Florists:

Title: Wedding florist inquiry

Hi,

My name is John and my partner Adam and I are engaged to be married on January 2019. We're looking for traditional arrangements for the wedding ceremony itself, the tables at the rehearsal and event dinner and an overall design of the venue. From what we saw on your website, we think you may just be the one to help us realize the dream of a perfect wedding. Getting down to business, what is the price range we should expect, and are there any additional services you supply, that may be of interest to us?

In addition, are there any non-custom arrangements that you make, and how much do you think such arrangements might lower the overall expenses?

Thank you for your time, I look forward to hearing from you.

Best Regards,

John Downy

2nd Wave: Opposite Sex

Bakeries:

Title: Cake Services

Hello,

My fiancée Ashley and I were searching for special wedding cakes and came across your website and we found it very impressive. We are looking for someone who can make us a 2 or 3 leveled cake with a custom made topper. Is that a wish you believe you could help us fulfill? Can you provide a rough estimate of how much the requested type of cake should cost?

Do you also sell stock cakes, which we can simply pick in store? Do you provide additional services for weddings and events? Our wedding will take place in 8 months (January 2019). We know that January 2019 is quite a ways away, but because it's a pretty busy time, we would like to begin the process sooner rather than later.

I look forward to your response.

Best,

Dylan

Photographers:

Title: Photography Service

Hello,

My fiancée Ashley and I were searching for wedding photographers and came across your website and we found it very impressive.

We were wondering if it would be possible to see a full wedding album or two?

We would like to check your availability, during April, 2019 and the pricing of your provided services. We know that April 2019 is quite a ways away, but because it's a pretty busy time, we would like to begin the process sooner rather than later.

What does your standard weekend package include and how much do you charge for it?

Are there any other services you provide?

I look forward to your response and wish you could help us make it a wonderful wedding.

Best,

Scott

Florists:

Title: Flower arrangement service

Hello,

My fiancée Ashley and I were searching for wedding flower arrangements and came across your website and we found it very impressive. We are engaged to be wed on January, 2019, and we were wondering if you would be available to help us?

We know that January 2019 is quite a ways away, but because it's a pretty busy time, we would like to begin the process sooner rather than later and hope you could help us make it a wonderful wedding.

In a nutshell, we're looking for flower arrangements for the entire duration of the event (general venue decorations, the wedding ceremony, the rehearsal dinner, and the party) that are not too flashy.

Can you provide a rough estimate of how much the requested service should cost?

Do you also sell stock decorations/arrangements, which we can simply pick in store? Do you provide additional services for weddings and events?

I look forward to your response.

Best,

Dylan

3rd Wave: Same Sex

Bakeries:

Title: February wedding - cake

Hi $%name%,
Paul and I are getting married in February next year and it seems like it's time to find a bakery for our wedding cake. We know it might be a little early but we wanted to check if you're available on February 22nd, 2019? Also, how much does a cake usually cost for about 120-140 guests? We know that different cakes have different costs, so can you please send us a few different options and we will go from there.
Most importantly, is it possible to come in for tastings? Unfortunately, this week doesn't work for us, but we are planning on running a few wedding errands in the next couple of weeks and wanted to coordinate a good day for this.
Thank you very much
Bob

Best Regards,

Robert Wood
[Phone number]

Photographers:

Title: Wedding photography

Hi $%name%,
Harry and I are getting married in May and are finally starting to plan. I hope it's not too late to ask if you are available to photograph on May 17-18, 2019?

If yes, could you tell us please what are your prices? Obviously, we realize there are different prices for different types of shoots, length of time etc. If you could send us a few different options with prices that would be great.

Ideally, if time and price work, we'd like to meet. This week we are really busy, but afterwards, we are planning on taking a day off work to run wedding errands and we would like to stop in to meet you and take a look at your work.

Thanks,
John

Best Regards,

John Downy
[Phone number]

Florists:

Title: February wedding - florist

Hi $%name%,

Paul and I are getting married in February and have decided it's time to start planning. Are you available on February 22nd, 2019? It sank in on us that arranging for flowers is a big deal, and we are still not sure what exactly we need. The essentials seem to be bouquets, boutonnieres, and ceremony flowers. Also, how much do you charge for flower arrangements per table? (in case it matters, we plan to marry at my parents' house and we expect about 120-140 guests).

We would love to see a few different pricing options and we can go from there. We would also love to see your arrangements in person, should we stop by the store or an event you are doing? If so, when works? We are planning on taking a day off of work in the next couple of weeks to run a few wedding errands and want to coordinate a good day for this.

Thank you very much

Bob

Best Regards,

Robert Morris
[Phone number]

3rd Wave: Opposite Sex

Bakeries:

Title: Wedding cake search

Hey $%name%,

My name is Scott and me and my fiancé Becca are engaged to be married.

We began our search for a wedding cake—for starters, are you available on March 1st, 2019? If yes, several brief questions:

I've been told the price is per slice. We're expecting 100 guests. How much do you charge for that? What's your menu of options for cakes?

Second, Becca has some general ideas in mind and we were thinking it would make most sense to coordinate a time to stop by. Do you have an appointment system? When would be good?

Thanks,

Scott

Scott Wilson
scottwils1990@gmail.com
[Phone number]

Photographers:

Title: Scheduling a meeting regarding a wedding in June

Hey $%name%,

I'm writing to check if you might be available to meet this Friday or the next Friday regarding a wedding consultation. My fiancée and I are getting married (whoo-hoo!) and looking for a photographer who might be free on the first weekend of June 2019.

Jessica, my fiancée, is obviously the one who is going to be the mastermind here. She has several ideas that she'd like to discuss about the style of photography. We also wanted to know what your price range is and whether you also do video.

Looking forward to hear more details from you, and what date and time might work for us to meet.

Thanks,

Dylan

Dylan Mitchell

dylanmitchell923@gmail.com
[Phone number]

Florists:

Title: Wedding florist search

Hey $%name%,

My fiancé Becca and myself are engaged to be married on March 1st, 2019. We would like to keep it simple, what flowers do you need for a wedding? And are you available on our date?

Here is what we came up with so far, would appreciate some price information for each item:

- bouquets for the bride and her maids

- Decorations, arrangements, whatever one need for the altar

- Reception flowers

Now, Becca has some general ideas in mind, would it make sense to coordinate a meeting at this stage?

Thanks,

Scott

4th Wave: Same-Sex

Bakeries:

Title: February wedding - cake

Hi $%name%,
Paul and I are getting married in February next year and it seems like it's time to find a bakery for our wedding cake. We know it might be a little early but we wanted to check if you're available on February 22nd, 2019? Also, how much does a cake usually cost for about 120-140 guests? We know that different cakes have different costs, so can you please send us a few different options and we will go from there.
Most importantly, is it possible to come in for tastings? Unfortunately, this week doesn't work for us, but we are planning on running a few wedding errands in the next couple of weeks and wanted to coordinate a good day for this.
Thank you very much
Bob

Best Regards,

Robert Wood

[Phone number]

Photographers:

Title: Wedding photography

Hi $%name%,
Harry and I are getting married in May and are excited to start planning! I hope it's not too late to ask if you are available to photograph on May 17-18, 2019?

If yes, could you tell us please what you offer? Obviously, we realize there are different prices for different types of shoots, length of time etc. If you could send us a few different options that would be great.

Ideally, if time and price work, we'd like to meet. We are planning on taking a day off work to run wedding errands next week or the one after. Would either Thursday the 28th or Friday the 29th work for you? And same for 7/5-6.

Thanks,
John

Best Regards,

John Downy
[Phone number]

Florists:

Title: February wedding - florist

Hi $%name%,

Paul and I are getting married in February and have decided it's time to start planning. Are you available on February 22nd, 2019? It sank in on us that arranging for flowers is a big deal, and we are still not sure what exactly we need. The essentials seem to be bouquets, boutonnieres, and ceremony flowers. Also, how much do you charge for flower arrangements per table? (in case it matters, we plan to marry at my parents' house and we expect about 120-140 guests).

We would love to see a few different pricing options and we can go from there. We would also love to see your arrangements in person, should we stop by the store or an event you are doing? If so, when works? We are planning on taking a day off of work in the next couple of weeks to run a few wedding errands and want to coordinate a good day for this.

Thank you very much

Bob

Best Regards,
Robert Morris
[Phone number]

4th Wave: Opposite-Sex

Bakeries:

Title: Wedding cake search

Hey $%name%,

My name is Scott and me and my fiancé Becca are engaged to be married.

We began our search for a wedding cake—for starters, are you available on March 1st, 2019? If yes, several brief questions:

I've been told the price is per slice. We're expecting 100 guests. How much do you charge for that? What's your menu of options for cakes?

Second, Becca has some general ideas in mind and we were thinking it would make most sense to coordinate a time to stop by. Do you have an appointment system? When would be good?

Thanks,

Scott

Scott Wilson
scottwils1990@gmail.com
[Phone number]

Photographers:

Title: Scheduling a meeting regarding a wedding in June

Hey $%name%,

I'm writing to check if you might be available to meet regarding a wedding consultation. My fiancée and I are getting married (whoo-hoo!) and looking for a photographer who might be free on the first weekend of June 2019 (June 1st).

Jessica, my fiancée, is obviously the one who is going to be the mastermind here. She has several ideas that she'd like to discuss about the style of photography. We also wanted to know your price range and whether you also do video.

Looking forward to hear more details from you, and what date and time might work for us to meet.

Thanks,

Dylan

Dylan Mitchell
dylanmitchell923@gmail.com
[Phone number]

Florists:

Title: Wedding florist search

Hey $%name%,

My fiancé Becca and myself are engaged to be married on March 1st, 2019. We would like to keep it simple, what flowers do you need for a wedding? And are you available on our date?

Here is what we came up with so far, would appreciate some price information for each item:

- bouquets for the bride and her maids

- Decorations, arrangements, whatever one need for the altar

- Reception flowers

Now, Becca has some general ideas in mind, would it make sense to coordinate a meeting at this stage?

Thanks,

Scott

[Phone number]

Section OA2 – Participant Exclusions

Reason for exclusion	Experiment	Control
Did not receive one email or more (we received an unsuccessful delivery message from the server). Happened due to either service failure or blocking or a wrong address (inexistent vendor).	**54**	**5**
Explicit suspicion	**8**	**1**

Removed due to an administrative error of the research staff that could have invoked suspicion (e.g., the sender referring to himself using a wrong name)	**4**	
Removed due to spillover (when vendor A referred to vendor B and forwarded him/her our original email) that could have invoked suspicion or contaminate the results.	**2**	
Total	**68**	**6**

Section OA3 – the Phone Survey

After sending the 3rd wave of emails (on June 13th), a random sample of emailed vendors were chosen to be surveyed on the phone. The calls were conducted between June 18th-21st 2018 (that is, before and shortly after the 4th wave emails were sent on June 20th) by two native English speakers RAs who identified as the senders of the 3rd wave email.

The purpose of the phone survey was twofold. The primary purpose was to gather data from previously non-responsive vendors. Non-responsive vendors were defined either as vendors who did not reply to the second email—the cause of the 2nd wave attrition—or vendors who did not reply to either email—potentially non-email-friendly vendors. Getting more insight on both types of non-response could be important. First, 2nd wave non-responders could have dropped from the experiment for a particular reason, such as suspicion, email fatigue, or some other reason, which a phone conversation could reveal. Second, complete non-responders might be aversive of email communication, a concern worthy of examination.

Following the recommended practice of randomly sampling non-respondents to supplement data collection,[1] we randomly sampled ~25-30% of non-responders; ~10% of past responders (vendors who responded to the second email, to compare them with those who did not, see Table 2; and vendors who responded to at least one of the emails, to compare them to complete non-responders, see Table 1); and 15% of control group vendors. Tables 1 and 2 describe the breakdown of the phone survey sample along each of these comparisons. Randomization was stratified based on the couple's identity (equal numbers of straight and gay callers), legal regime, and type of business.

The subsidiary purpose of the phone survey was to reduce attrition between the 3rd and 4th waves by putting a voice to the email and thickening the email correspondence, hopefully encouraging vendors to engage in future email-initiated communication. Since calling all 1,155 vendors was not feasible logistically, we relied on the random sampling detailed above.[2] In furtherance of the goal of preventing attrition, we also returned the calls of all 28 vendors who called the couple themselves (the 3rd email's signature included a phone number, and

[1] Alan S. Gerber and Donald P. Green, FIELD EXPERIMENTS: DESIGN, ANALYSIS, AND INTERPRETATION 236-238 (2012).
[2] *Id* (recommending random sampling as a more efficient method of supplement data collection).

some vendors called this number).[3] We distinguish between these four groups (previous non-responders; previous responders; control; returned calls) in analyzing the results of the phone survey. In summary, the phone survey sample was constructed as follows:[4]

Table OA3.1 - Phone Survey Sample Breakdown by complete vs. partial non-response

	Non-Responsive to neither 1st nor 2nd emails		Responsive to at least 1 of the 2 emails		Control – did not receive 1st and 2nd emails		Total	
# of vendors in group	205		699		251		1,155	
# of vendors in the survey Randomized | call backs	64 (31%)	3	76 (11%)	17	37 (15%)	8	177 (15%)	28

Table OA3.2 - Phone Survey Sample Breakdown by 2nd wave non-response vs. response

	Non-Responsive to 2nd email		Responsive to 2nd email		Control – did not receive 1st and 2nd emails		Total	
# of vendors in group	372		532		251		1,155	
# of vendors in the survey randomized | call backs	96 (26%)	7	44 (8%)	13	37 (15%)	8	177 (15%)	28

Method

An online account was opened using the CallHippo app.[5] Each character was assigned a US phone number (the same number included in its respective email signature) and a matching voicemail was recorded (e.g., "Hi, you've reached Scott, please leave a message"). Each character's outgoing call was performed from its assigned number.

Two native English speakers conducted the phone calls. Scripts for the conversation were prepared in advance, based on several moot calls with wedding vendors in non-participating states (e.g., New York) and were practiced with the speakers in advance. The scripts outlined the desired progress of the conversation, including different branches based on anticipated variance in vendors' reactions. The scripts also included all relevant information on the

[3] Three additional vendors who were part of the sample called the couple on their own initiative before being called; these are included in the counts of the random sample. In addition, the research team texted with 5 vendors who texted the number after conversation ended.

[4] 19 vendors who were originally part of the phone survey were ultimately excluded from the sample, as described in the appendix (15 due to failures in email delivery and 4 due to suspicion). Therefore, they are not included in the table and not analyzed as part of the phone survey data, although we do discuss the suspicious vendors below. Excluding these vendors did not change any of the results.

[5] https://callhippo.com/

speaker and his partner: the character's full name, the partner's name, the names of the parents, their hometown, and so on.

The speakers' main goal in each conversation was to determine the extent to which the vendor seemed keen to do business with the couple (on a scale of 1 to 10). Secondly, the speaker sought to understand whether the vendor was willing to schedule a face-to-face meeting and, if possible, get a price estimate of the service.

Results

Out of the 177 vendors randomly selected to participate in the phone sample, 25 (14%) have not indicated a phone number on their website or the number that was indicated was wrong. Returning the calls of 28 more vendors resulted in 180 called vendors. Each vendor was called at least twice, if not reached in the first attempt. Where possible, voice messages were left for non-answering vendors, resulting in 16 such messages.[6]

Seventy-three vendors answered our calls. Ultimately, 50% of the vendors who were called were reached either via call (41%) or voice message (9%). These rates were lower for vendors who were randomized to receive a call (48% reached of the 152 with valid numbers) than vendors who called the couple on their own initiative (57% reached of the 28 call-backs). The higher success rate in the latter group is expected given the self-selection into phone communication, yet the small size of this group prevents making strong inferences.

Phone responsiveness (who answered the call, regardless of the content of the answer) is reported by the two email non-response categorizations. First, businesses who did not reply to the 2nd wave email were also less likely to answer the phone (36%) than businesses who responded to that email (52%), suggesting that the 2nd wave attrition resulted from some businesses being generally less communicative than others—across modes of communication—rather than email fatigue or suspicion.[7] Second, response rates were identical for complete non-responders (vendors who responded to neither wave 1 nor wave 2 emails) and for vendors who responded to at least one of the emails (~40% each) and for vendors in the control group (49%[8]), suggesting that differences between these groups were not due to mode of communication. Specifically, complete non-response to emails does not appear to be driven by 'phone favoritism' of some sort, as email non-responders did not differ from email responders in their phone responsiveness. It is possible that some of the complete non-responders suffered from unobserved email failures. Four of them said during the call that they did not receive the 3rd wave email, and one of these vendors replied after the call that he found the email in the Spam folder. Two complete non-responders said that they saw

[6] It is possible that some of the calls failed because of some unknown number of vendors unintentionally provided wrong numbers (we can only determine whether vendors provided wrong numbers if an automated message is heard or if someone else answers the call and states that the number is wrong). However, it seems unlikely that such error would be pervasive, as vendors have an incentive to be reached by discerning customers.

[7] All incoming calls were from U.S. phone numbers. While some people might be more suspicious of unfamiliar numbers, it is unlikely that wedding vendors screen calls based on number unfamiliarity, as all customers are unfamiliar before they become customers.

[8] While the result for the control group seems higher, it is actually based on a small number of answered calls (19/39). For example, if 3 vendors in this group would not have answered the phone the response rate of the control group would have dropped to 41%.

the email but did not respond, thinking it was a scam (these vendors were excluded from the study as reported in the exclusion appendix). Other vendors did not explain 3rd wave non-response, or provided various reasons for not responding, e.g., having intended to respond or being unable to provide the service.

The initial response rate was identical for gay and straight callers (41% each)—as expected given that a phone number does not indicate the sexual orientation of the customer (unlike the email, which content made sexual orientation immediately transparent). The response rate of bakers (50%) and florists (47%) were virtually identical, whereas photographers were slightly less likely to answer the phone (34%), possibly related to photographers' lower likelihood of answering emails.

With the caveat of the small size of the actual resulting conversations (as noted, only 73 of the vendors picked up the phone), a few general observations can be noted. The average motivation to serve among vendors who answered the phone was 7.76 (on a 1-10 scale) and 56% of the vendors who answered the call were willing to meet the couple. The motivation to serve the opposite-sex couple was slightly higher (Mean=8.00, SD=3.123) than the motivation to serve the same-sex couple (Mean=7.55, SD=3.046) but the result was non-significant due to the small sample size (p=.4).[9] Meeting responsiveness (which is presumably conditioned on high motivation to serve) was roughly identical for both identities. Only 38% of conversations included price quotes, rendering any analysis of these quotes futile. Reasons for service refusal varied. One vendor responded to the gay caller that the business "only does traditional weddings". Other vendors mentioned unavailable dates and other reasons. Five vendors said that they closed their business (e.g., due to medical condition or relocation).

Finally, with respect to the subsidiary goal of the phone survey, comparing the email response rate of randomly called vendors in the 3rd and 4th waves, we found that called vendors were much more likely to respond to the 4th wave email. Including (excluding) control vendors, their response rate rose from 13% (11%) to 38% (37%), whereas the rest of the sample experienced a 5% attrition between the two waves. The increased retention had no impact on the main experiment results due to the double randomization pursued: each emailed vendor was randomly contacted by a same-sex or an opposite-sex couple in the 3rd wave (and vice versa in the 4th wave) and called vendors were randomly sampled out of the two identity groups, such that the positive retention effect equally spread across groups.

[9] Among the 55 vendors who answered the phone but did not reply to the 3rd wave email, 88% responded positively to the straight speaker, compared with 83% to the gay speaker.

Section OA4 – Additional Analyses of the Results

1. The impact of *Masterpiece* on Businesses that Previously Agreed to Serve Same-Sex Couples – Regression Analyses and Robustness Checks

Table OA4.1 – The Impact of *Masterpiece* on Businesses that Previously Provided Service to Same-Sex Couples (Linear Regressions)

	Agreement to Provide Service (Binary)				
	(1)	(2)	(3)	(4)	(5)
Same-Sex	-0.092***	-0.092***	-0.092***	-0.084***	-0.092***
	(0.021)	(0.021)	(0.021)	(0.024)	(0.021)
AD		-0.082*	-0.100**	-0.237***	-0.108**
		(0.045)	(0.045)	(0.059)	(0.045)
RFRA		-0.051	-0.049	0.051	-0.049
		(0.041)	(0.041)	(0.047)	(0.041)
AD*RFRA		0.169***	0.176***	0.310***	0.175***
		(0.062)	(0.062)	(0.074)	(0.062)
Wave 4			-0.029	-0.020	-0.029
			(0.021)	(0.024)	(0.021)
Photographer			-0.085***	-0.087**	-0.089***
			(0.032)	(0.037)	(0.032)
Republican Vote Rate					-0.150
					(0.144)
Constant	0.755***	0.776***	0.846***	0.737***	0.924***
	(0.019)	(0.030)	(0.038)	(0.046)	(0.085)
Business Fixed Effects	Yes	Yes	Yes	Yes	Yes
Only cities (80k+)	No	No	No	Yes	No
Businesses	575	575	575	485	575
Observations	1,150	1,150	1,150	932	1,150

Notes: The table reports the results from a series of linear regression analyses predicting the agreement of previously gay-friendly businesses to serve couples post-*Masterpiece*. All analyses include fixed effects at the level of the business. The dependent variable is binary response: 1 for positive response, 0 for negative or no response. Column 1 reports a simple regression that includes only the coefficient of sexual orientation (Same-Sex = 1 for same-sex couples and 0 otherwise). Column 2 adds the coefficients of legal regime (AD = 1 if an AD law exists in the jurisdiction and 0 otherwise; RFRA = 1 if a RFRA exists in the jurisdiction and 0 otherwise; AD*RFRA = 1 if the two laws coexist in the jurisdiction and 0 otherwise). Column 3 adds the coefficients of the experimental wave (wave 4 = 1 if the inquiry was sent in the second wave after the decision and the last wave in total, and 0 otherwise) and the type of business (Photographer = 1 is the business provided photography services and 0 if the business provided cakes or flowers).[10] Column 4 repeats the third analysis in a sample that is limited to businesses in urban areas. Column 5 reports the third analysis in the entire sample, controlling for the rate of voters for Republican candidates in the level of the county. $^{***}p < .01$, $^{**}p < .05$, * $p < .01$.

[10] No covariate for florists was included because of the small number of florists in the sample (N=82), their concentration in one regime, and an analysis that did not find them to be statistically significantly distinguished from bakers (see Table 8).

Table OA4.2 - The Impact of Masterpiece on Businesses that Previously Provided Service to Same-Sex Couples (Logistic Regressions)

	Agreement to Provide Service (Binary)				
	(1)	(2)	(3)	(4)	(5)
Same-Sex	-0.449***	-0.453***	-0.457***	-0.458***	-0.430***
	(0.131)	(0.132)	(0.132)	(0.132)	(0.143)
AD		-0.389**	-0.483**	-0.525***	-0.971***
		(0.184)	(0.188)	(0.192)	(0.233)
RFRA		-0.247	-0.241	-0.244	0.174
		(0.174)	(0.175)	(0.175)	(0.184)
AD*RFRA		0.831***	0.875***	0.871***	1.349***
		(0.265)	(0.266)	(0.267)	(0.299)
Wave 4			-0.146	-0.146	-0.099
			(0.132)	(0.132)	(0.143)
Photographer			-0.429***	-0.452***	-0.436***
			(0.139)	(0.141)	(0.152)
Republicans				-0.758	
				(0.620)	
Constant	1.127***	1.232***	1.599***	1.999***	1.144***
	(0.097)	(0.141)	(0.186)	(0.379)	(0.205)
Business Fixed Effects	Yes	Yes	Yes	Yes	Yes
Only cities (80k+)	No	No	No	No	Yes
Businesses	576	576	576	576	485
Observations	1,152	1,152	1,152	1,152	932

Notes: This table is identical to the former table, but analyzes the binary response measure in a logistic regression, presenting odds ratios instead. ***p < .01, **p < .05, * p < .01.

Table OA4.3 - The Impact of Masterpiece on Businesses that Previously Provided Service to Same-Sex Couples (Nuanced Response)

	Agreement to Provide Service (Nuanced)				
	(1)	(2)	(3)	(4)	(5)
AD		-0.076	-0.099*	-0.112**	-0.196***
		(0.054)	(0.054)	(0.055)	(0.068)
RFRA		-0.017	-0.015	-0.016	0.090*
		(0.050)	(0.050)	(0.050)	(0.054)
Same-Sex	-0.102***	-0.102***	-0.101***	-0.101***	-0.091***
	(0.026)	(0.026)	(0.026)	(0.026)	(0.029)
Wave 4			-0.046*	-0.046*	-0.029
			(0.026)	(0.026)	(0.029)
Photographer			-0.108***	-0.115***	-0.120***
			(0.039)	(0.039)	(0.042)
Republicans				-0.237	
				(0.175)	
AD*RFRA		0.150**	0.159**	0.157**	0.250***

		(0.076)	(0.075)	(0.075)	(0.086)
Constant	0.706***	0.712***	0.805***	0.929***	0.694***
	(0.023)	(0.037)	(0.046)	(0.103)	(0.053)
Business Fixed Effects	Yes	Yes	Yes	Yes	Yes
Only cities (80k+)	No	No	No	No	Yes
Businesses	576	576	576	576	485
Observations	1,152	1,152	1,152	1,152	932

Notes: This table presents the same models as the previous tables, but the dependent variable is the nuanced response measure (from -1, an explicit negative response, to +1, an explicit positive response). ***p < .01, **p < .05, * p < .01.

Tables OA4.1-3 report the regression results of the main analysis of the experiment results, treating the main dependent variable of agreement to serve the couple as binary (3.1, 3.2) or nuance (4.3), subjecting it to a linear (4.1, 4.3) and a logistic (4.2) regression analysis. As the tables show, the results are consistent across these modeling approaches and specifically the negative effect of *Masterpiece* on previously gay-friendly businesses is highly significant in all models and approaches.

Column 1 in all tables estimates the impact of sexual orientation as the sole factor influencing the agreement to serve the couple. Column 2 adds the coefficients for the legal regime in which businesses are located (these results are discussed in Section S4.5 below).

Columns 3-5 are robustness checks and follow up studies. Specifically, in Column 3 I control for the additional experimental variables – the wave of inquiry and type of business. The results are robust to both.[11] Then, Columns 4 and 5 examine whether the *Masterpiece* effect is more or less pronounced in regimes that vary in their conservativeness. I examine this question from two angles.

First, I limit the analysis to cities which population is larger than 80,000 people. This check evaluates the potential concern that the results are driven by rural and less populated areas, where conservative attitudes towards same-sex marriage are presumably more prevalent. If this was the case also at the time of the experiment, these areas could have tilted the results towards discrimination of same-sex couples. Another reason to examine urban areas in particular is the common argument that sexual orientation discrimination is less of a problem in larger cities. Accordingly, Column 4 analyzes only businesses located in larger cities. The *Masterpiece* effect is robust to this analysis. The negative effect of *Masterpiece* is not less pronounced in urban areas and does not appear to be driven by rural areas (otherwise, their exclusion would have eliminated the effect). The same results emerge from the analyses of the entire sample of businesses, the control group, and the particularly keen

[11] The tables show that photographers were less likely to agree to provide service than bakers and florists, across couple types (see also Table OA4.6 comparing the experiment and control groups). Notably, this result was not causally linked to *Masterpiece* as photographers were less likely to provide service both before and after *Masterpiece*. Before *Masterpiece*, photographers provided -7.6% positive responses (p = .002), +5.4% non-response (p = .019), and +1.6% negative responses (p = .18) than other vendors. After *Masterpiece*, photographers provided -7.6% positive responses (p = .003), +4.3% non-response (p = .066), and +2.6% negative responses (p = .06) than other vendors. These findings are consistent with the hypothesis articulated in Section 3.1 that photographers might be pickier in general about their customers as they are more intimately involved in the wedding than bakers and florists.

businesses (Tables OA4.2, 4.3, and 4.5 below). In all these analyses, the *Masterpiece* effect remains robust to the focus on urban areas.

A different and perhaps more straightforward proxy of conservativeness is the percent of the population who voted for the Republican Presidential candidate. Table 6, column 5, includes a covariate for the Republican vote rate (average of the last three elections, 2008-2016) in the county from which the business operates.[12] The results of the regression results do not lend support to the hypothesis that the *Masterpiece* effect is more pronounced in more conservative areas, as the coefficient of the Republican vote rate is not statistically significant. The same results emerge from the analyses of the entire sample of businesses, the control group, and the particularly keen businesses (Tables OA4.2, 4.3, and 4.5 below).

Overall, the results from both the big cities analysis and the Republican vote analysis suggest that the *Masterpiece* effect is not explained by the conservativeness of the environment in which businesses operate. Note that this null result is not very surprising given that the sample was constructed to minimize political and attitudinal differences to facilitate the legal regime comparison. I deliberately chose jurisdictions that are similar politically and demographically (but differ in their legal regimes), as shown in Table 1. This methodological design limited the span of the conservativeness of the environments in the study, what could have attenuated the role of this factor in the results.

[12] Data from *U.S. General Election Presidential Results by County From 2008 to 2016*, GITHUB, https://github.com/tonmcg/US_County_Level_Election_Results_08-16 (last modified Sep. 7, 2018).

2. The impact of *Masterpiece* on the entire experiment group

This analysis includes the entire sample of businesses in the experiment group (N=904 businesses * 4 observations per business, resulting in 3616 observations). While it provides a complete picture of the results, the analysis is less indicative of the *Masterpiece* effect because of the significant drop in responsiveness (attrition) that occurred between waves 1 and 2, reducing the overall rate of responses to opposite-sex couples. Still, Table OA4.4 indicates a drop of 14.4 pp in positive responses to same-sex couples after *Masterpiece*, or ~23 percent.

Table OA4.4 - Average Response Rates of Businesses Before and After Masterpiece, by Sexual Orientation of Couples

		Negative Responses		No Response	Positive Responses			Percent Diff. in		
Period	**Couple**	*Neg.*	*Neg. w/ Referral*		*Coop.*	*Pos.*	**Total PR**	**PR OS-SS**	**Total RR**	**N**
Before *Masterpiece*	Opposite-Sex	2.9	2.8	41.3	3.1	50.0	53.1	-10.49	58.7	906
	Same-Sex	4.0	3.3	29.1	3.9	59.7	63.6	(0.0013)	70.9	906
After *Masterpiece*	Opposite-Sex	4.8	4.0	33.7	1.6	56.1	57.6	8.39	66.3	906
	Same-Sex	6.3	3.4	41.1	1.4	47.8	49.2	(0.0009)	58.9	906

Table OA4.4 also indicates a statistically significant increase in negative responses for same-sex couples after the decision (p = .001), from 10.28% negative responses before *Masterpiece* (66 out of 642 responses) to 16.5% negative responses after *Masterpiece* (88 out of 534 responses; a 6.2 pp difference). While the trend is in the same direction for opposite-sex couples, the parallel increase in negative responses for opposite-sex couples was smaller and not statistically significant at $p < 0.05$ (from 9.8% to 13.3%; a 3.5 pp difference; p = .064). With the caution that this analysis is influenced from the fact that most vendors did not bother to provide an explicitly negative response, the increase in negative responses for same-sex couples after *Masterpiece* is 177% the increase of such responses for opposite-sex couples (p = .03).

Table OA4.5 reports the analysis of the entire sample, which yielded a highly significant and negative coefficient on the interaction between the court's decision (Post Court=0 for measurements prior to the decision and 1 for measurements after the decision) and sexual orientation (Same Sex=0 for a heterosexual couple inquiry and 1 for a same-sex couple inquiry). Namely, businesses were less likely to agree to serve same-sex couples after *Masterpiece*. This is despite the fact that businesses were, on average, more likely to respond positively to couples after *Masterpiece* and that the coefficient of Same Sex was positive (these are both artifacts of week 2 attrition problem[13]).

[13] The sharp attrition in week 2 both reduced the average rate of response before *Masterpiece* as compared with after *Masterpiece*, and simultaneously reduced the overall rate of response to heterosexual couples as compared with same-sex couples, because all emails in week 2 came from heterosexual couples. As a result, both the Post Court and Same Sex coefficients were significant and positive.

Table OA4.5 - The Impact of *Masterpiece* on Agreement to Provide Service to Same-Sex and Opposite-Sex Couples (All businesses)

	Agreement to Provide Service			
	(1)	(2)	(3)	(4)
AD		-0.145***	-0.162***	-0.096
		(0.036)	(0.036)	(0.062)
RFRA		-0.129***	-0.122***	-0.147***
		(0.035)	(0.035)	(0.042)
Post Court	0.045***	0.045***	0.045***	0.046**
	(0.017)	(0.017)	(0.017)	(0.020)
Same Sex	0.105***	0.105***	0.105***	0.114***
	(0.017)	(0.017)	(0.017)	(0.020)
Photographer			-0.080***	-0.105***
			(0.027)	(0.032)
AD*RFRA		0.237***	0.242***	0.172**
		(0.051)	(0.051)	(0.071)
Post Court*Same Sex	-0.189***	-0.189***	-0.189***	-0.190***
	(0.024)	(0.024)	(0.024)	(0.028)
Constant	0.531***	0.608***	0.660***	0.695***
	(0.016)	(0.027)	(0.032)	(0.042)
Business Fixed Effects	Yes	Yes	Yes	Yes
Only cities (80k+)	No	No	No	Yes
Businesses	904	904	904	632
Observations	3,616	3,616	3,616	2,528

Notes: The table reports the results from a series of linear regression analyses predicting the agreement of all businesses in the experiment to serve couples post-*Masterpiece*. All analyses include fixed effects at the level of the business. The dependent variable is binary response: 1 for positive response, 0 for negative or no response. Column 1 reports a simple regression that includes only the coefficient of sexual orientation (Same-Sex = 1 for same-sex couples and 0 otherwise) and Post Court (1 = After Masterpiece, 0 = Before Masterpiece), and the interaction term (1 = Same Sex Couple After Masterpiece, 0 otherwise). Column 2 adds the coefficients of legal regime (AD = 1 if an AD law exists in the jurisdiction and 0 otherwise; RFRA = 1 if a RFRA exists in the jurisdiction and 0 otherwise; AD*RFRA = 1 if the two laws coexist in the jurisdiction and 0 otherwise). Column 3 adds the coefficients of the type of business (Photographer = 1 is the business provided photography services and 0 if the business provided cakes or flowers). Column 4 repeats the third analysis in a sample that is limited to businesses in urban areas. $^{***}p < .01$, $^{**}p < .05$, * $p < .01$.

More descriptively, the results indicate a sexual orientation gap in the positive responses to couples after *Masterpiece* which ranges from 7 to 10 percentage points. Specifically, in the first week after *Masterpiece*, 60% of the vendors who randomly received an inquiry from a heterosexual couple agreed to provide service to that couple as compared with 50% of those who randomly received an inquiry from a same-sex couple (10 percentage points gap). On the second week after *Masterpiece,* the randomization flipped such that each vendor received an email from the counter-orientation couple. Of the businesses now contacted by heterosexual couples, 55 percent responded favorably, as compared with 48 percent of those contacted by

same-sex couples (7 percentage points gap). Note that this pattern indicates both between-subject differences (in each week, between the random groups) and within-subject differences (across weeks, within each group). Note that this pattern indicates both between-subject differences (in each week, between the random groups) and within-subject differences (across weeks, within each group). Weekly attrition cannot explain the within-subject pattern, as the rate of response went *up* in in the group that received the first message from a same-sex couple and the second message from a heterosexual couple. Weekly attrition cannot explain the within-subject pattern, as the rate of response went *up* in in the group that received the first message from a same-sex couple and the second message from a heterosexual couple. Controlling for all other factors—legal regime, business type, county conservativeness, and urban environment – showed that the effect is robust.

3. Comparing the experiment and the control groups post-*Masterpiece*

One may be concerned that the detected effect arises from the repeated measurement of businesses rather than from the *Masterpiece* decision itself. To evaluate this concern, in this analysis I compare the post-*Masterpiece* results in the experiment group to the results in the control group (N=251 businesses), which was contacted for the first time after *Masterpiece*. This analysis also helps evaluating the effect of the attrition that occurred in the experiment group on the overall results (see below).

Notably, the makeup of the control group is not identical to that of the experiment group, yet businesses in this group were sampled and measured following exactly the same procedure applied to the experiment group post *Masterpiece*. While the control group is not powered to detect differences between legal regimes, it is large enough to detect the two effects of interest in this analysis: First, the effect of the repeated measurement on the experiment group, i.e. whether business behavior change as a result of being contacted repeatedly in the experiment. A comparison with the control group, which is freshly contacted after Masterpiece, can answer this question. Second, and crucially, whether the repeated measurement effect influenced the *Masterpiece* effect. To put in other words, did the experiment *itself* cause the pattern of discrimination which is evident post-*Masterpiece*? This might happen if businesses become fatigued or suspicious in non-random fashion that disproportionately influences same-sex couples and not opposite-sex couples. The interaction term of two variables: Control (0 if Experiment group and 1 if Control group) and Same Sex (0 is opposite-sex and 1 if same-sex) can indicate whether same-sex discrimination post *Masterpiece* is unique to the experiment group or extends to the control group.

Table OA4.6, column 1, answers the first question—is there an effect for the repeated measurement of businesses? I find that the repeated measurement influenced the results, such that businesses in the control group were significantly more likely to respond favorably to couples of all identities as compared with businesses in the experiment group. However, this result is not robust, as indicated from the comparison of the effect in row 2 across the different models. This comparison indicates that the difference between the Control and the Experiment groups disappear in models 4 (controlling for legal regime), 5 (only cities) and 6 (controlling for county conservativeness, as proxied by the percent of Republican voters).

Row 3 in Table OA4.6 answers the second question—did the experiment itself produce the pattern of discrimination evident post-*Masterpiece*? As the table shows, the answer is negative. Being in the control or in the experiment group did not interact with the effect of sexual orientation: although the baseline rate of positive responses in the two groups somewhat differed, businesses in the control group were just as likely to discriminate against same-sex couples post-*Masterpiece* as businesses in the experiment group (Experiment averages: 57 percent provided positive response to opposite-sex couples v. 49 percent who did the same for same-sex couples; Control averages: 66.5 percent versus 57 percent, respectively).

Table OA4.6. Impact of *Masterpiece* on Agreement to Provide Service in the Experiment and Control Groups

	Agreement to Provide Service					
	(1)	(2)	(3)	(4)	(5)	(6)
Same Sex	-0.084***	-0.084***	-0.084***	-0.084***	-0.076***	-0.084***
	(0.017)	(0.017)	(0.016)	(0.016)	(0.020)	(0.016)
Control	0.089**	0.062*	0.083**	0.052	0.040	0.049
	(0.035)	(0.036)	(0.035)	(0.036)	(0.039)	(0.037)
SameSex*Control	-0.012	-0.012	-0.012	-0.012	-0.018	-0.012
	(0.035)	(0.035)	(0.035)	(0.035)	(0.038)	(0.035)
Week4			-0.025*	-0.025*	-0.015	-0.025*
			(0.015)	(0.015)	(0.017)	(0.015)
AD		-0.142***		-0.152***	-0.106	-0.156***
		(0.039)		(0.039)	(0.066)	(0.039)
RFRA		-0.136***		-0.132***	-0.142***	-0.132***
		(0.034)		(0.034)	(0.040)	(0.034)
AD*RFRA		0.257***		0.265***	0.217***	0.262***
		(0.052)		(0.052)	(0.075)	(0.052)
Photographer			-0.048*	-0.055**	-0.071**	-0.057**
			(0.026)	(0.026)	(0.030)	(0.026)
Republican Vote Rate						-0.074
						(0.113)
Constant	0.576***	0.651***	0.618***	0.697***	0.712***	0.736***
	(0.016)	(0.027)	(0.024)	(0.032)	(0.040)	(0.068)
Business Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Only cities (80k+)	No	No	No	No	Yes	No
Businesses	1155	1155	1155	1155	866	1155
Observations	2,310	2,310	2,310	2,310	1,732	2,310

Notes: The table reports the results from a series of linear regression analyses predicting the agreement of businesses in the experiment and control groups to serve couples post-*Masterpiece*. All analyses include fixed effects at the level of the business. The dependent variable is binary response: 1 for positive response, 0 for negative or no response. Column 1 reports a simple regression that includes only the coefficient of sexual orientation (Same-Sex = 1 for same-sex couples and 0 otherwise) and Control (1 = Control group, 0 = Experiment group), and the interaction term (1 = Same Sex Couple + Control group, 0 otherwise). Column 2 adds the coefficients of legal regime (AD = 1 if an AD law exists in the jurisdiction and 0 otherwise; RFRA = 1 if a RFRA exists in the jurisdiction and 0 otherwise; AD*RFRA = 1 if the two laws coexist in the jurisdiction and 0 otherwise). Column 3 adds the coefficients of the experimental wave (wave 4 = 1 if the inquiry was sent in the second wave after the decision and the last wave in total, and 0 otherwise) and the type of business (Photographer = 1 is the business provided photography services and 0 if the business provided cakes or flowers; the control group included only photographers and florists). Column 4 repeats the third analysis in a sample that is limited to businesses in urban areas. Column 5 reports the third analysis in the entire sample, controlling for the rate of voters for Republican candidates in the level of the county. $^{***}p < .01$, $^{**}p < .05$, * $p < .01$.

4. The Impact of *Masterpiece* on businesses that were generally keen to provide service prior to the decision

As a robustness check, I re-run the main analysis on the group of businesses that agreed to provide service to *both* same-sex and opposite-sex couples before *Masterpiece* (N=422 businesses * 2 post *Masterpiece* observations per business, resulting in 844 observations). As in the main analysis, the examination focuses on how these businesses behave after the *Masterpiece* decision. There are several reasons why I consider this analysis as subsidiary to the main analysis. First, this cohort treated both same-sex and heterosexual couples favorably before *Masterpiece*. Hence, on the one hand, it presents an opportunity to examine whether *Masterpiece* had a negative effect on businesses that before the decision appeared to be equal treatment businesses. On the other hand, this cohort does not appear to exhaust the actual group of equal treatment businesses in the general sample. Businesses self-select into this group in part based on their responses to the second wave of emails, because this is the wave of inquiries from opposite-sex couples; as I explained in section XX in the article, this wave suffered from significant reduction in responsiveness.[14] In these conditions, businesses that responded positively to both couple types before *Masterpiece* appear to be characterized by particular responsiveness and general eagerness to provide service, in addition to being egalitarian. This characterization is confirmed by the findings of the random phone survey, described in Section OA3, that indicate that businesses that did not respond to the second wave of emails were generally less responsive—also over the phone—than other businesses. Put in other words, the current analysis asks what effect did *Masterpiece* have on businesses that prior to the decision were highly keen to do business in general.

To answer this question, I created a dataset of all equal treatment businesses (N=422) and then examined their responses to same-sex and heterosexual couples after *Masterpiece*. The analysis has only two observations per business as behavior prior to *Masterpiece* is given.

Table OA4.7 plots the results, showing that the coefficient for sexual orientation (Same Sex) is significant and negative in all models. These results are particularly striking given that this group of businesses provided the same favorable treatment to same-sex and heterosexual couples before *Masterpiece* and appeared particularly responsive and keen to do business with everyone. Indeed, as anticipated, the overall rates of positive response of the keen businesses were higher than the general sample, such that on the first week after Masterpiece, 77% of the businesses contacted by same-sex couples responded favorably and in the second week, 74% responded the same. However, these response rates were considerably lower than the 86% who responded favorably to heterosexual couples on the first week (9 percentage points gap) and the 79% who responded the same on the second week (5 percentage points gap). The effect of sexual orientation on the keen (and previously egalitarian) businesses was independent from the effect of between-week attrition (Week4 coefficient).

[14] Because of wave 2 attrition this group is smaller than the group of businesses that provided service only to same-sex couples. As the event of reverse discrimination against heterosexual couples pre-*Mastepiece* seems unlikely, the group of seemingly equal treatment businesses potentially excludes businesses who are equal treatment businesses but were excluded due to wave 2's responsiveness problem.

Table OA4.7 - The Impact of *Masterpiece* on pre-*Masterpiece* Generally Keen Businesses

	Agreement to Provide Service				
	(1)	(2)	(3)	(4)	(5)
Same Sex	-0.069***	-0.069***	-0.069***	-0.066**	-0.069***
	(0.024)	(0.024)	(0.024)	(0.029)	(0.024)
AD		-0.089*	-0.097**	-0.163**	-0.107**
		(0.046)	(0.046)	(0.072)	(0.046)
RFRA		-0.059	-0.059	-0.074	-0.059
		(0.042)	(0.042)	(0.047)	(0.042)
Wave 4			-0.050**	-0.026	-0.050**
			(0.024)	(0.029)	(0.024)
Republican Vote Rate					-0.199
					(0.149)
Photographer			-0.055*	-0.082**	-0.063*
			(0.032)	(0.037)	(0.032)
AD*RFRA		0.161**	0.163**	0.220***	0.162**
		(0.064)	(0.064)	(0.084)	(0.063)
Constant	0.825***	0.855***	0.914***	0.938***	1.018***
	(0.020)	(0.031)	(0.038)	(0.048)	(0.087)
Business Fixed Effects	Yes	Yes	Yes	Yes	Yes
Only cities (80k+)	No	No	No	Yes	No
Businesses	422	422	422	302	422
Observations	844	844	844	604	844

Notes: The table reports the results from a series of linear regression analyses predicting the agreement of generally keen businesses to serve couples post-*Masterpiece*. All analyses include fixed effects at the level of the business. The dependent variable is binary response: 1 for positive response, 0 for negative or no response. Column 1 reports a simple regression that includes only the coefficient of sexual orientation (Same-Sex = 1 for same-sex couples and 0 otherwise). Column 2 adds the coefficients of legal regime (AD = 1 if an AD law exists in the jurisdiction and 0 otherwise; RFRA = 1 if a RFRA exists in the jurisdiction and 0 otherwise; AD*RFRA = 1 if the two laws coexist in the jurisdiction and 0 otherwise). Column 3 adds the coefficients of the experimental wave (wave 4 = 1 if the inquiry was sent in the second wave after the decision and the last wave in total, and 0 otherwise) and the type of business (Photographer = 1 is the business provided photography services and 0 if the business provided cakes or flowers). Column 4 repeats the third analysis in a sample that is limited to businesses in urban areas. Column 5 reports the third analysis in the entire sample, controlling for the rate of voters for Republican candidates in the level of the county. $^{***}p < .01$, $^{**}p < .05$, * $p < .01$

5. The Impact of Masterpiece in Different Legal Regimes – Full Analysis

As elaborated in Part 3 of the paper, businesses were sampled to the experiment from four regimes that are similar in economic, demographic, and attitudinal respects, but differ in how they regulate religious freedom and equality duties (see Table 1). Specifically, North Carolina has no RFRA and no AD law at any level of government. Iowa has no RFRA but has a state AD law. Both Indiana and Texas have state RFRAs and no AD laws, yet some local governments within these states enacted AD laws. One can expect that the effect of *Masterpiece* might differ between these regimes. In particular, businesses subject to AD laws can be expected to adopt their behavior post-*Masterpiece* if they believe that the decision has relaxed their antidiscrimination obligations. In contrast, businesses in regimes that have never enacted AD laws have no legal basis to change their behavior, as they were never obliged to not discriminate on the basis of sexual orientation to begin with.

Returning to the analysis of businesses that provided positive responses to same-sex couples before *Masterpiece* (Section OA4.1), Column 2 in Tables OA4.1-3 show that in addition to the highly significant and reliable effect of sexual orientation on the agreement to provide service, the coefficients for AD and for the AD*RFRA interaction are also statistically significant. (The RFRA coefficient is negative but not statistically significant in the vast majority of the models). These results were generally robust to different modeling approaches and to the inclusion of other variables, as columns 3-5 in all tables show.

To understand the interaction between AD and RFRA, I probe further into the differences between legal regimes in Table OA4.8. The results indicate that *Masterpiece* had a highly statistically significant negative effect on businesses' agreement to provide service to same-sex couples in all regimes, *except* regimes that enacted both an antidiscrimination law and a religious freedom law.

Table OA4.8 – Impact of *Masterpiece* on Businesses that Previously Responded Positively to Same-sex Couples, by Legal Regime

	Agreement to Provide Service (Binary)			
	+RFRA +AD	+RFRA -AD	-RFRA +AD	-RFRA -AD
Same Sex	-0.021	-0.101**	-0.108**	-0.127***
	(0.042)	(0.043)	(0.049)	(0.039)
Week4	-0.006	0.004	-0.126**	-0.014
	(0.042)	(0.043)	(0.049)	(0.039)
Republican Vote Rate	0.188	-0.276	0.059	-0.351
	(0.275)	(0.351)	(0.389)	(0.217)
Photographer	-0.088	-0.190***	-0.121	-0.039
	(0.059)	(0.067)	(0.093)	(0.057)
Florist			-0.154	
			(0.099)	
Constant	0.745***	0.995***	0.840***	1.003***
	(0.137)	(0.194)	(0.196)	(0.125)
Business Fixed Effects	Yes	Yes	Yes	Yes
Businesses	139	148	115	173
% of Regime Businesses	65%	61%	55%	73%
Observations	278	296	230	346

Notes: This table reports the effect of Masterpiece on businesses operating in different legal regimes,

that prior to the decision agreed to provide services to same-sex couples. AD+RFRA regimes are counties in IN and TX that are subject to state RFRAs and have enacted local AD laws; -AD-RFRA regimes are counties in IN and TX that are subject to state RFRAs and have not enacted local AD laws; the AD-RFRA regime comprises of all counties in IA, a state that has enacted an AD law and no RFRA; the -AD-RFRA regime comprises of all counties in NC, a state that has not enacted a RFRA nor an AD law, and had no county with such laws at the time of the experiment. Because florists were concentrated in the +AD-RFRA regime this is the only regime with such covariate. $^{***}p < .01$, $^{**}p < .05$, * $p < .01$.

The first notable contrast is between Column 1 (RFRA+AD) and Column 2 (RFRA-AD), each including businesses from Indiana and Texas. Both states enacted RFRAs, yet AD laws were enacted, if at all, only at the municipal level. Consequently, the state of the law varies *within* those states. *Masterpiece* had a negative effect on same-sex couples in jurisdictions that did *not* prohibit sexual orientation discrimination, but not in jurisdictions that enacted AD laws. The negative effect of *Masterpiece* in jurisdictions where businesses had no legal basis to change their behavior is also evident in Column 4 (-RFRA-AD), which includes North Carolina businesses. Although no law prohibited these businesses from refusing to serve same-sex couples before *Masterpiece*, businesses in this regime that previously agreed to serve same-sex couples shift to discrimination after the decision.

The second notable contrast is between column 1 (RFRA+AD), including businesses from Indiana and Texas, and column 3 (-RFRA +AD), including businesses from Iowa. Both regimes prohibit discrimination on the basis of sexual orientation, yet Indiana and Texas also enacted state RFRAs, whereas Iowa did not enact a RFRA or a similar statute at any level of government. As noted in Section 3.1, businesses in both regime types were expected to adopt their behavior post-*Masterpiece*. Yet only businesses in the AD regime (Iowa) adopt their behavior post-*Masterpiece*, whereas businesses in +RFRA+AD regimes (Texas and Indiana)—that were expected to adopt their behavior even more—do not significantly differentiate on the basis of sexual orientation after *Masterpiece*.

6. The Impact of *Masterpiece* as a function of Religious Environment - Additional Analyses

Table OA4.8. The Effect of *Masterpiece* and Religious Density on pre-*Masterpiece* Gay-Friendly Businesses

	Agreement to Provide Service				
	(1)	(2)	(3)	(4)	(5)
Same Sex	-0.092***	-0.092***	0.008	-0.092***	0.019
	(0.021)	(0.021)	(0.049)	(0.021)	(0.051)
AD	-0.100**	-0.127***	-0.127***	-0.105**	-0.105**
	(0.045)	(0.047)	(0.047)	(0.045)	(0.045)
RFRA	-0.049	-0.075*	-0.075*	-0.082*	-0.082*
	(0.041)	(0.044)	(0.044)	(0.046)	(0.046)
Week4	-0.029	-0.029	-0.029	-0.029	-0.029
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Photographer	-0.085***	-0.087***	-0.087***	-0.089***	-0.089***
	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
Evangelical Congregations Density		-84.005*	-14.25		
		(48.378)	(57.377)		
Religious Congregations (all groups) Density				-52.730*	-6.645
				(31.58)	(36.93)
AD:RFRA	0.176***	0.206***	0.206***	0.187***	0.187***
	(0.062)	(0.064)	(0.064)	(0.062)	(0.062)
Same Sex*Evangelical Density			-139.51**		
			(61.699)		
Same Sex*Religious Density					-92.17**
					(38.28)
Constant	0.846***	0.925***	0.875***	0.928***	0.872***
	(0.038)	(0.059)	(0.063)	(0.062)	(0.066)
Business Fixed Effects	Yes	Yes	Yes	Yes	Yes
Businesses	576	576	576	576	576
Observations	1,152	1,152	1,152	1,152	1,152

Notes: The table reports on the relationship between religious environment and agreement to provide service to couples of different types, focusing on businesses that provided positive responses to same-sex couples before *Masterpiece*. The basic full model that includes all experimental and legal covariates, but no religious covariates, is included for ease of reference in column 1. Column 2 reports accounts for Evangelical density, i.e. the ratio between the number of Evangelical communities in the county where a business is located and the population of the county. Column 3 adds the interaction term between Evangelical density and Same-Sex. Columns 4 and 5 report the same models while accounting for Religious density, i.e. the ratio between the number of congregations of all religious groups and the population of the county. Data on religious and Evangelical density is taken from the U.S. Religion Census: Religious Congregations and Membership Study, 2010, referenced in the main manuscript. Note that differences of scale make the coefficients of religious and Evangelical density hard to interpret.***p < .01, **p < .05, * p < .01.

Section OA5 – The Coding Process

Two RAs coded the entire dataset of emails, closely supervised by the PI. One RA coded the entire photographer's data and the other coded the entire bakeries and florist's data. The research team conducted multiple meetings throughout the coding process to discuss the coding method, resolve open issues, and fine-tune the coding scales.

There are four observations per vendor in the experiment group (two before and two after *Masterpiece*) and two observations per vendor in the control group (after *Masterpiece*). Each observation (a row in the coding sheet) represents an email correspondence between a specific vendor and a specific potential customer. An email correspondence may include anything from no response to several responses. Most of the data for each observation is based on the first response from the vendor ("main email").[15] Subsequent emails in the same correspondence (same wave) were not considered in most coding variables; because the research team replied to each vendor based on the content of their specific main email, subsequent emails were mostly incomparable.

The coded variables:

Nuanced response:

A response scale of 5 options:

1 for an explicit positive response (agrees to provide service),

-1 for an explicit negative response (declines to provide service),

and **0** for no response at all. Nuanced responses received different values;

an implicit positive response received **0.5** (e.g., asking for additional information about the location of the venue; we interpreted this response as a tentative agreement to provide service, which is not dependent on the sexual orientation of the couple).

A response was coded as **-0.5** when the vendor declined to provide the requested service but referred the couple to a colleague, or suggested other products, or alternative dates.[16]

Binary response:

A dichotomization of Nuanced Response: **1** for 1 or 0.5. **0** for 0, -0.5 or -1.

Price Per Hour (PPH) (in USD): photographers price variable.

To the extent that photographers mentioned prices, the prices they offered varied greatly in their form of presentation. Many photographers had some array of "packages" that included a combination of varying quantity of shooting hours and additional services for a fixed price.

[15] The two only exception to this rule is first, when a vendor sent a non-automated acknowledgment that she had received the inquiry and will reply shortly, such email was considered in the Time and Cordiality variables. Second, a special variable counted the number of follow-ups – defined as emails sent after the first email.

[16] We considered the suggestion of an alternative date a negative response because it is common knowledge that a wedding date will not be changed for any single provider, making the suggestion merely a matter of politeness. In waves 1 and 2, where the email noted the month of the wedding and did not note a specific date, we coded any agreement to provide service anytime within this month as 1.

Price per hour of photo shooting was defined as the *price for the cheapest package offered* divided by the *number of shooting hours the package included*. There are 732 such data units out of 1,780 responses from photographers (~41%). Additional 226 data units did not include these variables but included other pricing data and their PPH was extrapolated based on a formula explained below.[i] Because of the small number of data points relative to the sample, this variable ultimately lacked sufficient data to be analyzed.

Price Per Serving (PPS) (in USD): bakers price variable.

Price per serving is the most common pricing method in the wedding cake industry. If a vendor priced the cake without mentioning PPS, we calculated the PPS by dividing said price by the average number of guests that was noted in the inquiry letter. For example, if the inquiry letter mentioned 120-140 guests, and the vendor cited a price of $X for the cake, PPS is 130/X. The data includes 340 observations with PPS out of 799 non-empty (Nuanced Response ≠ 0) bakers' observations (~42%). Other replies from bakers did not mention price. Because of the small number of data points relative to the sample, this variable ultimately lacked sufficient data to be analyzed.

Cordiality:

This measure was coded based on the content and subtext of the correspondence[17] and aimed to capture the level of cordiality, warmth, politeness, and desire to do business with the couples, asreflected in the response, on a 5-unit scale:

Value	1	2	3	4	5
Definition	An indifferent or rude comment	A polite response that is seemingly indifferent as to whether we do business or not	A desire to do business, accompanied with a businesslike attitude	A desire to do business, accompanied with a cordial attitude	A strong desire to do business or a very cordial attitude
Example	"Call the shop" (and nothing else)	"you are welcome to check our website and see if you are interested"	"I would love to work with you and Adam"	"Hey Bob! Congratulations on your upcoming wedding! We would LOVE to work with you and Paul for your special day in February!"	"I don't mind driving to your neck of the woods to meet up if it's easier for your schedule"

The coding process for Cordiality included comparing each vendors' four main emails to understand each vendor's tone and style and capture nuanced differences between correspondences within-vendor. To facilitate the comparison between vendors, the same RA evaluated all vendors from the same industry. One concern regarded the comparison between photographers (coded by one RA) and bakers and florists (coded by another RA). To minimize this concern, the two RA's worked closely together, discussing ambiguities and refining the

[17] Considered in this variable is both the main email and the acknowledgment email, if existed.

coding on a day to day basis until the completion of the coding. They also went over and discussed cross-examples from each other's industries, together with and supervised by the PI. The coding process yielded the impression that photographers are generally more cordial than bakers and florists. It is therefore possible that some responses could have received a higher grade, coming from a baker/florist, than from a photographer, simply due to the contrast effect (against the backdrop of the industry). Ultimately, despite the significant amount of efforts invested in the coding of cordiality, the concerns about comparability and the potential subjectivity of the assessment process resulted in this variable not being analyzed.

Response Time (in minutes):

This variable capture the elapsed time between when the inquiry letter was sent and the receipt of the vendor's response.[18] It was computed only for vendors who replied to the inquiry letter (Nuanced Response ≠ 0). Times larger than 10k minutes (roughly a week) were excluded to prevent outliers (110 out of 2703 responses – less than 5%). I did not detect a significant difference in response times in the main analyses of the study and this variable was not analyzed further.

Additional variables that were created in the coding process but were not analyzed in the present project:

Length	Number of words in email.
Meeting	Did the vendor suggest a meeting in person? (binary)
Calling	Did the vendor suggest a phone call/skype call/face time? (binary)
Standard Pricing	Was the pricing, if included, standard (e.g., an attached pdf)? (binary)
Custom	Did the vendor suggest a custom package? (binary)
Acknowledgment	Did the vendor send a preliminary non-automated email (before the main email) to acknowledge receiving the inquiry? (binary)
Follow Up	How many emails did the vendor send after the main email?
Nuanced Discrimination	By-variable comparing waves 1 v. 2 and 3 v. 4, to capture subtle discrimination within subject, which was not captured in Cordiality; **1** for discrimination against same-sex couples, **-1** for discrimination against opposite-sex couples; missing in all cases captured in Cordiality.
Spouse Name	Did vendor mention the spouses' name? (binary)
Congratulating	Did vendor congratulate the couple? (binary)

[i] PPH extrapolation method:

[18] Response for this variable is defined as the main email unless an acknowledgment was sent (and then it's defined as the acknowledgment).

This method was used when the one or both of the two main pricing variables were unavailable, but some other pricing information was available. The original variations of price information we observed were the following:

1) Price for Cheapest Package (951 results)
2) Hours for Cheapest Package (803 results)
3) Price for most expensive package (800 results)
4) Price per Additional Hour (402 results)

Option A: Price for cheapest package (1) is available and Hours for Cheapest Package (2) missing (152 observations):

1) If the variable appeared in the paired week (1-2, 3-4), the missing value was substituted for the matching week value (e.g. if for some vendor the Hours for Cheapest Package were 6 in week #1 and week #2 was missing, then PPH for week #2 was calculated based on 6 hours, regardless of different values that may have been provided in weeks #3 and #4.
2) Alternatively, if the variable appeared in only one of the four weeks, the missing values in the other weeks were substituted for that value. (e.g., if Hours for Cheapest Package were 7 in week #1 and week #4 was missing, and there was no data on week #3, then PPH in week #4 was calculated based on 7 hours.
   Both options 1 and 2 were marked in **BLUE** in the data.
   NOTE: both above options assume discrimination in number of hours is unlikely.
3) If vendors described their package as "full," without specifying the exact number of hours, PPH was calculated based on **8** hours. If vendors described their package as "half," PPH was calculated based on 4 hours. This quantification is based on a few cases where the vendor gave an exact number of hours and referred to this number as "full". "Full" numbers varied between 6-10 hours, with 8 being the frequent answer.
4) If neither of the options above appeared in the data, PPH was calculated based on the average Hours for Cheapest Package in the sample (**6.37** hours).
   Both options 3 and 4 were marked in **GREEN** in the data.

Option B: Price for cheapest package (1) is missing and Price for Additional Hour (4) is available (27 results):

Phase 1 – calculating the average difference between PPH and Price for Additional Hour for all vendors that have all three variables 1,2,4. The average difference is **106.84 $**. (PPH is more expensive than price for additional hour – the reason for this is that PPH represents the price of a work hour plus a portion of the fixed costs – creating the album, meetings etc.)

Phase 2 – PPH is defined as:

$$PPH = Price\ for\ Additional\ Hour + 106.84\left[\frac{\$}{hour}\right]$$

EXHIBIT E

UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION

Chelsey Nelson Photography LLC and **Chelsey Nelson,** Plaintiffs, **v.** **Louisville/Jefferson County Metro Government; Louisville Metro Human Relations Commission-Enforcement; Louisville Metro Human Relations Commission-Advocacy; Verná Goatley,** in her official capacity as Executive Director of the Louisville Metro Human Relations Commission-Enforcement; and **Marie Dever, Kevin Delahanty, Charles Lanier, Sr., Leslie Faust, William Sutter, Ibrahim Syed,** and **Leonard Thomas,** in their official capacities as members of the Louisville Metro Human Relations Commission-Enforcement, Defendants.	**Case No. 3:19-cv-00851-BJB-CHL** **Rebuttal Expert Report of George Yancey, Ph.D.**

I have been retained by Plaintiffs Chelsey Nelson Photography LLC and Chelsey Nelson, through their counsel, Alliance Defending Freedom, to serve as a rebuttal expert in *Chelsey Nelson Photography LLC v. Louisville/Jefferson County Metro Government*, Case No. 3:19-cv-00851-BJB-CHL. I submit the following report in rebuttal to Professor Netta Barak-Corren's June 30, 2021 report in this matter.

I. Educational Background and Professional Credentials.

1. I am a tenured Professor of Sociology at Baylor University with a joint appointment in the Department of Sociology and the Institute for Studies of Religion. I received my Ph.D. in Sociology from the University of Texas at Austin in 1994, my M.A. in Economics from the University of Texas at Austin in 1989, and my B.A. in Economics from West Texas State University in 1985.

2. To date, I have authored or co-authored 14 academic books and thirty-nine peer reviewed articles. I have co-authored a book (Yancey and Brunson 2018) explaining how individuals in the media shape stories according to their racial, gender, and religious biases. I have also published on other subjects including media and academic bias, institutional racial diversity, racial identity, and anti-Christian attitudes. I have been published in the *American Sociological Review*, the top-rated journal in sociology. Another of my publications won the Charles J. Miller Christian Scholar's Award which is given to the best article of the year in *Christian Scholar Review*.

3. I bring special expertise to the topics raised by Professor Barak-Corren's report as it concerns the potential effect on Christian communities. This expertise comes from my peer-reviewed work on anti-Christian hostility (Yancey and Williamson 2014, Yancey 2010) and the treatment of Christians by those with high levels of cultural power (Yancey and Brunson 2018, Yancey 2011). I have been recognized as one of the few scholars who have studied anti-Christian bias in the

United States (Shellnutt 2017) and may arguably be the most prominent expert on that subject.

4. I also have experience in producing peer-review qualitative and quantitative empirical work and have acted as a reviewer for the top sociological journals, including *American Journal of Sociology* and *Social Forces*. This experience includes, but is not limited to, assessing experimental techniques, determining the viability of qualitative methodology to comprehend the actions of social actors, and applying statistical analysis to the understanding of sociological phenomenon. I also have used audit research (defined below) in my previous publication concerning media bias (Yancey and Brunson 2018). In that book we sent out surveys with contrasting potential media stories to see if reporters would react differently if certain racial, sexuality, religious or political characteristics were altered in those stories. This work informs my awareness of the strengths and weaknesses of audit approaches as well as the way bias affects the way media stories are presented.

5. A fuller review of my professional experience, publications, and awards is provided in my curriculum vitae, a copy of which is attached as Exhibit A. In addition to the publications listed in Exhibit A, I have published on the following websites: https://www.patheos.com/blogs/shatteringparadigms/2017/07/welcome-to-shattering-paradigms/; https://www.christianitytoday.com/edstetzer/2020/august/white-fragility-order-of-unity.html; https://www.christianitytoday.com/ct/2015/march-web-only/what-christianophobia-looks-like-in-america.html; https://stream.org/california-universities-religious-discrimination-problem/; https://spiritualdirections.tumblr.com/post/626881579992662017/not-white-fragilitymutual-responsibility; https://www.thepublicdiscourse.com/author/george-yancey/; and https://www.thegospelcoalition.org/profile/george-yancey/.

6. This is my first engagement to provide expert services in connection with litigation. I am being compensated for my services at a rate of $200/hour. My compensation does not depend in any way on the outcome of this litigation, or the opinions stated herein.

7. In this rebuttal report, I provide my expert views, with reference to my own scholarship, other academic literature, and other cited materials, on two questions:

- Does Professor Barak-Corren's report support the conclusion that a judicially created religious exemption for Plaintiffs' photography and editing services from Louisville's Metro Ordinance (§ 92.01, et seq.) will significantly increase the likelihood that same-sex couples will be denied services because of their sexual orientation when attempting to hire photographers in Louisville, Kentucky?
- Does Professor Barak-Corren's report adequately account for the potential costs to religious freedom if Louisville's Metro Ordinance (§ 92.01, et seq.) is allowed to force Plaintiffs to create photographs and blogs to celebrate same-sex marriage in violation of their religious beliefs?

II. Summary of Opinions.

8. In my opinion, Professor Barak-Corren's report cannot determine the potential effect on same-sex couples of a religious exemption from Louisville's Metro Ordinance for Plaintiffs' photography and editing services. I explain the basis for this opinion in Section III below. It is also my opinion that Professor Barak-Corren's report does not sufficiently account for the converse of her conclusions, i.e., how the lack of a religious exemption for Plaintiffs in this case could limit religious freedom and how the loss of that freedom will impact the larger society. I explain the basis for this opinion in Section IV below.

9. In developing my opinions in this matter, I reviewed and/or relied on Professor Barak-Corren's report and her forthcoming papers Netta Barak-Corren, A License to Discriminate? The Market Response to *Masterpiece Cakeshop*, 56(2), Harvard Civil Rights-Civil Liberties Law Review (forthcoming 2021) ("HCRCL") and Netta Barak-Corren, Religious Exemptions Increase Discrimination Towards Same-sex Couples: Evidence from *Masterpiece Cakeshop*, Journal of Legal Studies (forthcoming 2021) ("JLS").[1] I also reviewed and/or relied on Professor Barak-Corren's online appendix and anonymized data and code (available at https://osf.io/ve5yn/?view_only=8853549b0fc248afb793ef41d4e953f8). I also reviewed and/or relied on Plaintiffs' Complaint. Furthermore, I reviewed and/or relied upon the academic articles and other materials cited in this report and/or listed below as references as well as my background as researcher in forming my opinions.

III. The Report cannot assess the potential impact of a ruling in favor of Plaintiffs on discrimination against same-sex couples in Louisville, Kentucky.

10. The Report is unable to show that same-sex couples in a specific area (Louisville, Kentucky) will be adversely impacted by a court ruling in favor of Plaintiffs with the current data. The Report uses an audit study design. An audit study is a type of study used to test for potential discrimination where investigators pose as fictitious customers who differ by one key factor, send requests to experimental units, and then the study compares treatment and control groups on a dichotomous outcome. For example, in the Report's study, auditors differed by sexual orientation (the key factor), sent requests to wedding vendors (the

[1] For clarity, I will refer to Professor Barak-Corren's report in the remainder of this rebuttal as "the Report." When I reference the "Report" I am referring to her written report, her article A License to Discriminate? The Market Response to *Masterpiece Cakeshop*, and her article Religious Exemptions Increase Discrimination Towards Same-sex Couples: Evidence from *Masterpiece Cakeshop*.

experimental united), and measured responses (the dichotomous outcome). The key in audit studies is to keep all characteristics of the fictitious customers the same and only change the one factor being tested. That way the test can determine if the experimental units react differently to the changed characteristic.

11. But the Report here used multiple contacts to the same vendors. This creates several problems with the audit. First, there is the possibility of a fatigue effect where the experimental units (e.g., wedding vendors), no longer respond to the audit. As I explain below, this may have led to a problem of attrition between waves 1 and 2 of the Report, a problem which significantly undermines the Report's findings.

12. Second, repeated audits may allow the sampled wedding vendors to suspect they are being studied. For example, Barak-Corren noted twelve instances of explicit or implicit suspicion and identified several additional instances of suspicion in follow-up telephone calls. (Appendix pp. 10-11, 13-14). Despite Barak-Corren's attempts to account for those possibilities, it is not possible to completely control for the effects of the problems identified in paragraphs 11 and 12 because of the study's design.

13. A third problem is the way the Report contacts the same business multiple times, specifically that Barak-Corren must fashion different emails for each hypothetical couple. (Appendix p. 1-10). This is problematic because the use of different emails in Barak-Corren's study introduces the potential problem that vendors react differently to the contrasting ways the emails are worded. In my research using an audit study (Yancey and Brunson 2018), I handled this problem by including enough respondents so that I could provide half of the respondents with the treatment condition and half with the control condition. But other than the experimental treatment, my audit study used the same documents. Other researchers have tried to alleviate the problem posed by different content in

requests by rotating many different content among the respondents. This minimizes the impact that minor variations in only two inquires (for example, the use of a general date in waves 1 and 2 and a specific date in waves 3-4 in Barak-Corren's study) has on the experimental unit's outcome. (Neumark, Bank, and Van Nort 1996).

14. The issue of content differences is compounded by Barak-Corren's strategy to send out the emails in waves. For example, emails to photographers in wave 1 (emails from purported same-sex couples) asked for the services to be provided in April generally. (Appendix p. 2). But emails to photographers in waves 3 and 4 from purported same-sex couples identified a specific date in May for their proposed wedding. (Appendix pp. 4-8). Then Barak-Corren coded a suggestion of a different date in wave 1 as a positive response but coded a suggestion of a different date in waves 3 and 4 as a negative response. (Appendix p. 29 n.16). The change in the date for the requested services combined with the coding differences in the responses compound the problem of the change in the email content. We cannot know how the vendors would have reacted to identical emails where the only difference was whether the couple was the same or different sex. An approach that is more appropriate and more widely accepted in the field of audit studies would have been for Barak-Corren to send out emails from same-sex testers to half of the vendors and emails from opposite-sex testers to the other half of the vendors. Then after the *Masterpiece* decision, she should have sent out identical emails of the opposite type of couple to those wedding vendors (so that a wedding vendor who received an email from a same-sex couple before *Masterpiece* would receive an email from an opposite-sex couple after *Masterpiece* and vice versa). As it stands, these problems alone make it difficult to have any confidence in the Report's findings since different wording and timing in emails likely affected the propensity of a wedding vendor to respond to an email.

15. The analysis in the Report is bivariate, meaning that the Report tests two variables: the purported customer's sexual orientation and the vendor's response. Barak-Corren provides a multivariate analysis (meaning an analysis measuring more than one outcome variable, like AD law, RFRA, Republican vote rate) in the Appendix to try to determine whether the relative power of a single variable (i.e., sexual orientation) affects a given statistical relationship (the likelihood of a positive response). (Appendix pp. 15-17, 20, 23, 25). But one of the assumptions of the linear or logistic regression analysis used in the Appendix is that the vendors' characteristics are not related to each other. When I say a regression analysis, I mean the method for measuring the relationship between a dependent variable (in this case a positive or negative response to providing a service for a same-sex wedding) and one or more independent variables (such as whether the vendor lives in an AD state or RFRA state, Republican vote rates, etc.). This assumption is violated, though, when drawing cases from only four states and using the characteristics of those states (i.e., +/- AD and +/- RFRA) as potential independent variables. Because the vendors come from the same states and vendors within the same jurisdictions share AD/RFRA characteristics, the vendors are "nested."

16. A nested dataset means that the subject data come from the same cluster (e.g., students in a classroom or vendors from the same geographical region). When you have a nested dataset, the researcher cannot run a linear or logistic regression analysis because those analyses assume the individuals' independence. But vendors nested in the same geographic cluster are more likely to behave the same way—and therefore be more dependent. For example, by coming from the same jurisdiction, these vendors share social contexts in a variety of ways, such as socialization patterns, traditions, attitudes, outlooks, and professional goals.

17. To account for this issue, the research must use some type of multilevel modeling to disaggregate the clustered subjects from their cluster. For example, in

the Report, a multilevel model could include assessment of the data on multiple levels such as the level of the individual (level 1), possibly the level of the county or municipality (level 2), and the level of the state (level 3). This would make any inferences more accurate by reducing standard errors and increasing the chances of locating more significant relationships. Stated differently, a multilevel model would increase the accuracy of predicting whether certain variables (i.e., sexual orientation of the purported customer, income level of the wedding vendor, etc.) are significant in determining likely positive or negative responses. By failing to use a multilevel model, the conclusions in the Report possibly overestimate the potential impact of a ruling in Plaintiffs' favor by downplaying the possible significance of independent variables other than a couple's sexual orientation.

18. I also question how much we can assert about the effect of same-sex status on the customer's ability to receive services given the relatively low number of independent variables in the Report's regression models. The Report relies on discussing a few state-level characteristics (religiosity, attitudes towards homosexuality, Republican vote) to assess a particular metropolitan area (Louisville). (e.g., Report ¶¶ 19-23). But it is risky to rely on so few variables to predict what will occur in Louisville. We simply do not know if those variables sufficiently explain the results. Without the inclusion of other independent variables (e.g., income level, education, percent of population that are sexual minorities, racial makeup) in a robust multivariate model, we can only speculate about the potential of discrimination against same-sex couples in Louisville. A multilevel regression model would provide important information such as whether the effect of different types of couples can be explained by differences in other variables. This is critical if we are going to make assertions about whether these results indicate whether the status of same-sex couples leads them to face discrimination. Are the number of variables used by Barak-Corren sufficient for us

to have confidence that she has captured alternative explanations of differential treatment by the couples? It is impossible to say because the regression models did not have any measurement of coefficient of determinants. So I could not determine how much of the variation in responses was due to the other variables which could affect response rate. Coefficient of determinants tell us the degree to which vendors' willingness to respond to emails is tied to the independent variables in the models such as the sexual orientation of the couple or some other variable (i.e., income of wedding vendor, education level of wedding vendor, etc.). In other words, coefficient of determinants would help to explain how one factor (i.e., a positive or negative response) can be caused by its relationship to another related factor (i.e., sexual orientation of the couple or income, race, or political affiliation of the wedding vendor). The higher the coefficient, the more likely that relationship would lead to a particular conclusion. Do the variables in Barak-Corren's model (i.e., sexual orientation, political population, religiosity) explain half of that willingness? Ten percent? Half of one percent? We cannot know without any measurement of coefficient of determinants.

19. My inclination is to include as many independent variables as possible regardless of the size of coefficient of determinants. The exclusion of other social and demographic measurements (such as income level, education, percent of population that identify as LGBT, or support for LGBT issues) indicates that we will not know if those other variables explain the relationship between a couple's same-sex status and their ability to find a wedding vendor. For example, it may be that certain metropolitan areas contain a high percentage of African-Americans who tend to be more religious than whites (Smith et al. 2002, Yancey 2005) and may be less supportive of sexual minorities (Lewis 2003, Glick and Golden 2010). Without racial controls, we do not know if the results in the Report would be mediated with the inclusion of a racial makeup variable. Nevertheless, we cannot

determine the degree to which religiosity or attitudes towards homosexuality matter in the potential effects observed in the current data because the Report does not account for additional variables and cannot assess the particular results in Louisville.

20. But even if the Report included sufficient independent variables and utilized the proper statistical techniques (which it does not), we still cannot state that we know what will happen in Louisville. We would be limited in making assessments of how a ruling in favor of Plaintiffs will impact individuals in this particular metropolitan area. Sociological methodology does not allow us to determine what will happen in a particular case. It only allows a prediction, and that prediction will always be incomplete. For example, if you told me a person was five feet ten inches tall and asked me to guess the sex of that person, I would say that this person is a male. Regression models can show that men are taller than women even after all appropriate controls (such as income and nutrition content) have been utilized. Since five feet ten inches is taller than the average height for women, I will be right more times than wrong. But I could generally be wrong if the sample size included only women from the WNBA. I will also always be wrong if the person in question is Brittney Griner (a 6 foot 9 inches tall WNBA basketball player), Maria Sharapova (a 6 foot, 2 inches tall profession tennis player), my first adult girlfriend (who was 6 foot, 4 inches tall), or many other above-average woman. Similarly, solid systematic work can be predictive over a large number of metropolitan areas, but it is much less reliable when predicting the actions of persons within a specific city or county. The Report's findings, as applied to Louisville, are speculative and cannot be an accurate prediction of what will happen in Louisville in the event of a favorable ruling for Plaintiffs.

21. Beyond the statistical limitations, there are also methodological assumptions made by Barak-Corren in the Report that affect its usefulness in this particular

court case. One of the first problems is the short-term nature of the study. Barak-Corren argues that a change in social norms allows vendors to perceive service refusals to be more permissible (HCRCL pp. 38, 47-48; JLS p. 37), which allows even formerly "gay-friendly" businesses to decline wedding services to perceived same-sex couples. Yet this argument assumes that these attitudes will persist over time. But the Report's research design does not measure long-term effects. To the degree the Report measures attitudinal change, there is no guarantee that such a change will be lasting.

22. To illustrate the likely temporal nature of attitude changes, we can examine the literature on diversity training and prejudice reduction. In contrast to a single public event, individuals in diversity training are subject to an intense program with the aim to reduce their prejudice. Generally, there is a short-term reduction of prejudice. Yet meta-analysis research on prejudice reduction indicates that six months after such a program, individuals exhibit similar levels of prejudice than they had going into the program (Bezrukova et al. 2016).

23. Given what we know about the inability of diversity programs to produce long-term change, the burden is on the Report to show that a single court case will impact attitudes long-term. But if individuals are unlikely to alter their long-term racial prejudices after an intense program designed to produce such attitudinal alterations, then it is highly unlikely that a significant number of individuals alter their long-term attitudes after a single court verdict. To the degree that the Report has captured any attitudinal change, it is doubtful that this change is permanent or even long-lasting.

24. By contrast, an adverse ruling against Plaintiffs in this case could have an ongoing damaging impact on the religious freedom of religious groups who share Plaintiffs' views on marriage as I will assess in Section IV.

25. It should also be noted that there was not a differentiation of the effects between photographers, bakers, or florists in the bivariate presentation of Barak-Corren's research in Table 5. (JLS pp. 24-25). In her regression analysis, there is an attempt to measure whether photographers, bakers, or florists have different propensities to offer services in general. This measurement is noted in the tables as an "Agreement to Provide Service." (Appendix pp. 23-28). But I could not find a direct measurement of differences between photographers, bakers, and florists in their explicit declines to provide a service for a same-sex couple because of their sexual orientation. That is an important omission and requires the Report's conclusions to be based on the assumption that all types of wedding vendors respond to all requests for services equally because Barak-Corren equates a "no response" to a decline in waves 3 and 4. (HCRCL pp. 38-39; JLS p. 26). If explicit refusals of service are nearly identical between photographers, bakers, and florists though, then this should be part of the research. Such a finding would not be conclusive, but it would indicate that we have reason to believe that there is a similar effect across all wedding vendors. Without such information, it is unrealistic to believe that all different types of wedding vendors will treat all requests for same-sex weddings equally. In fact, Barak-Corren notes that vendors do not respond to requests for services equally. Barak-Corren notes that photographers "were less likely to agree to provide service than bakers and florists" and hypothesized that "photographers might be pickier in general about their customers as they are more intimately involved in the wedding than bakers and florists." (Appendix p. 17 n.11). But Barak-Corren assumes photographers, bakers, and florists respond to all requests similarly to predict an increase in same-sex discrimination across all wedding vendors. Without some evidence that wedding vendors will act in similar manners when processing requests for services, that assumption is not sustainable. Indeed, it is possible that there may be certain types

of vendors who prefer same-sex marriages to opposite-sex marriages and their inclusion would dramatically lower, or even possibly erase, any impact on same-sex couples.

26. There are also questions about whether the Report's experimental design reflects reality. An experimental design is when a researcher introduces a population to a situation that is normal (control group) and one that includes the characteristic the experimenter wants to learn about (treatment group). Experimental designs are useful because they allow the research to control for all possible effects except the one being studied. Indeed, as I explained above (paragraphs 11-14), part of my disappointment with Barak-Corren's treatment of the email messages is that by offering different emails from same-sex and opposite-sex couples, she allows for the possibility of different content to impact the results, instead of having a pure study concentrated on same-sex/opposite-sex differences.

27. However, even in the best of circumstances, the artificial nature of this experiment does not allow it to reflect reality. For example, this research is based only on email communication. At best, this research may indicate the potential actions of wedding vendors when contacted by emails. It is unclear how often a couple uses email as opposed to social media messaging or phone or face-to-face conversations to initiate a request for wedding services. Barak-Corren states that it is common to communicate with wedding vendors via email (HCRCL pp. 32-33), but I did not see any statistics on how often emails are the first contact for wedding vendors or any analysis about whether photographers, bakers, or florists have different preferences for the initial inquiry. Barak-Corren also assumes that same-sex couples will require multiple services to be obtained through cold calling different wedding vendors (HCRCL p. 7), but she supplies no evidence that this is the typical experience for wedding couples. Without such information, it is

impossible to calculate even a baseline prediction of the extent of discrimination same-sex couples may face.

28. Barak-Corren argues that using emails instead of real individuals in an audit study reduces the chances of accidental bias by real life auditors. (JLS pp. 20-21). However, it is quite possible that the email audit technique increases the possibility of false negatives. The classic work of LaPiere (1934) strongly suggests that individuals are more tolerant in person than they are through a remote medium. LaPiere sent mail correspondence to hotels to see if they would accept a Chinese guest. The vast majority (over 90 percent) of the hotels stated that they would not accept a Chinese guest. Yet when LaPiere went to the hotels with a Chinese couple only one of sixty-six hotels refused the couple a room. Mailing correspondence is a poor predictor of how individuals are likely to act. Accordingly, I would expect that email correspondence to be a poor predictor of how wedding vendors would react to face-to-face or phone contacts or direct social-media messaging. LaPiere's work suggests that more personal contacts are more likely to elicit a supportive response than an email inquiry. Barak-Corren's research supports LaPiere's conclusions. For example, of the wedding vendors who answered phone calls requesting services for a same-sex wedding, eighty-three percent of those who answered the phone responded favorably to the request and only one vendor (unidentified by service type) declined because of an objection to same-sex weddings. (Appendix p. 14 n.9).

29. I find the attrition (i.e., non-responses) between wave 1 and wave 2 to be problematic to the Report's conclusions as well. The response rates in waves 1 and 2 were 70.8% and 58.7% respectively. (HCRCL p. 36; JLS p. 21). Since the first wave inquired about services for same-sex couples and the second wave inquired about services for opposite-sex couples, this non-response cannot be due to hostility towards same-sex couples. Yet Barak-Corren codes non-responses in waves 3 and 4 as discrimination against same-sex couples. (HCRCL pp. 38-39; Appendix pp. 15,

20, 23, 25). She argues that the greater attrition (i.e., non-response) for same-sex couples in waves 3 and 4 is due to discrimination. (HCRCL pp. 38-39; Appendix pp. 15, 20, 23, 25). Yet same-sex couples were more likely to receive responses in wave 1 than opposite-sex couples in wave 2. Applying Barak-Corren's logic of attrition in waves 3 and 4 to waves 1 and 2 would lead to the conclusion that businesses discriminated more against opposite-sex couples than same-sex couples prior to *Masterpiece*. But this result calls into question the idea of using attrition (or non-response) as a measurement of discrimination. Since there are other reasons for possible non-response (differences in email language between waves, lost emails, failed business, change in response policies, business is overbooked, vendors vacation more in June when waves 3 and 4 were sent), it is possible that businesses that were more responsive to same-sex requests in wave 1 are more vulnerable to other reasons for attritions and thus were less likely to respond in waves 3 and 4. I note that Barak-Corren's telephone survey identified many reasons for non-responses, including "various reasons for not responding" such as "having intended to respond." (Appendix p. 14). The use of a more traditional audit study where half of the businesses received identical inquiries from the same-sex couple and half from an opposite-sex couple would have helped answer this question, but it is not possible to replicate that design from this point forward.

30. Barak-Corren argues that the *Masterpiece* case changes attitudes towards religious exemptions, making it more plausible for discrimination against same-sex couples to take place. (HCRCL pp. 38-40, 46-49). Yet the *Masterpiece* case wasn't a religious exemption case, and indeed the Supreme Court did not rule on the viability of a religious exemption. The *Masterpiece* ruling was based on the presence of religious hostility by government officials. So, to make her argument that religious exemptions may lead to an increase in sexual orientation discrimination, Barak-Corren cannot use the ruling in the case itself. Instead, Barak-Corren relies

on how the media reports about the case. (HCRCL pp. 25-27). Most notably, she cites conservative media sources such as FOX news and Catholic News Agency. But they are a minority of the media sources reporting on the case, as Barak-Corren acknowledges. (HCRCL pp. 25-27). Barak-Corren must rely on one of two assumptions to reach her conclusions.

31. First, Barak-Corren could be assuming that most media covered *Masterpiece* positively as a religious exemption case. But I do not think this assumption is justifiable. My research suggests that most of the media is not sympathetic to conservative Christians, generally defined as conservative Protestants (which would include Southern Baptists) who are likely to share Plaintiffs' beliefs about marriage. (Yancey and Brunson 2018). My co-author and I showed that when given competing scenarios, media personnel are more likely to emphasize free speech rights when speech is aimed at criticizing Christians than when free speech is directed at objecting to same-sex marriage. It is unlikely that general media coverage of the *Masterpiece* decision focused on a positive assertion of the right of conservative Christians to exercise their religious freedom. It seems more likely that most news coverage criticized the opinion or presented the case as a narrow opinion, not as a religious exemption case, and/or criticized the opinion. Barak-Corren even confirms that "[a]ll mainstream outlets" reported the decision as "narrow." (HCRCL p. 25). Barak-Corren's potential assumption that a few conservative media outlets created a generally favorable media atmosphere for conservative Christians and for broad religious exemptions goes against current research on religious media bias. (Kerr 2003, Kerr and Moy 2002, Yancey and Brunson 2018).

32. Second, Barak-Corren could be assuming that the vendors who responded negatively or with no-response in waves 3 and 4 were exposed only or primarily to conservative media sources. But this assumption is not justifiable either because Barak-Corren did not provide any evidence about which vendors were exposed to

what news coverage. Barak-Corren also does not differentiate the responses between vendors who consume conservative media sources and vendors who consume mainstream or liberal media.

33. In sum, for the reasons I have described above, I must conclude that the Report cannot determine the potential effect that a religious exemption from Louisville's Metro Ordinance for Plaintiffs' photography and editing services would have across all wedding vendor's willingness to provide services for same-sex couples in Louisville or anywhere else. However, the lack of such an exemption could come at considerable costs of religious freedom to the larger Christian communities, a factor Barak-Corren also did not adequately consider. I will do so in the next section.

IV. The Report does not assess the potential harm an adverse ruling against Plaintiffs may inflict on Christian vendors.

34. It is essential to weigh the issues of sexual-orientation antidiscrimination laws and religious freedom. Barak-Corren argues that a favorable ruling for Plaintiffs will change the social atmosphere in such a way as to make discrimination against same-sex couples by wedding vendors more acceptable. (HRCLC pp. 46-49). I question whether her analysis adequately measures this potential change in our social atmosphere and urge caution in using this assertion as a basis for a ruling, as I discuss in Section III. By contrast, it is also plausible that a ruling against Plaintiffs—meaning a court finding that the Metro Ordinance requires Plaintiffs to create photographs and blogs contrary to their religious beliefs—could threaten religious freedoms by making Christian communities vulnerable to attack. Barak-Corren does not consider this possibility.

35. Predicting how this case will affect the larger Louisville community with absolute certainty is not possible with even the best sociological techniques. However, Barak-Corren has derived her prediction based on either bivariate

analysis of type of couple and type of response (which cannot assess causality) or regression models that fail to account for other independent variables (income, race, etc.) and failed to use a coefficient of determination to measure causal connections, as I explained in Section III. In contrast, in my work, I rely on both qualitative and quantitative analysis. My assessment about the characteristics of those likely to reject Christians (Yancey and Williamson 2014) and my assertion that many individuals favor those in the LGBT community due to apathy towards Christians (Yancey 2018) are based on regression models using national probability data and use a reliable number of social and demographic independent variables as controls. The nature of that animosity and the willingness of those with it to take away the rights of Christians are shaped by qualitative analysis of hundreds of respondents (Yancey and Williamson 2014) and experimental research not burdened by the content and timing issues of Barak-Corren's work (Yancey 2013). Overall, the empirical evidence presents a stronger possibility of the potential detrimental effects on the Christian community than the evidence of potential detrimental effects on same-sex couples. For these reasons, as well as the additional reasons explained below, it is reasonable to argue that an adverse ruling against Plaintiffs could possibly lead to more loss of rights by Christians compared to the potential loss of services by same-sex couples in the event of a favorable ruling for Plaintiffs.

36. Recent research I have conducted indicates that some individuals favor members of the LGBT community due to their antipathy towards conservative Christians, as defined by the respondents in the study. (Yancey 2018). For example, in an academic book (Yancey 2013), I used a scenario in a survey asking respondents how much they would punish a Christian landlord who refused to rent an apartment to a same-sex couple. Nearly fifty percent of those that demonstrated hostility towards Christians wanted a maximum fine ($10,000) while only about fifteen percent of those that did not demonstrate hostility towards Christians

wanted the maximum fine. It is not surprising that those with animosity towards Christians are much more likely to pronounce the highest punishment possible when provided an opportunity to punish Christians. The support and implementation of sexual orientation anti-discrimination laws can provide individuals with anti-Christian animosity the opportunity to punish Christians. This is not to say that all individuals eager to enforce such laws have such animosity or that we cannot have laws to protect against sexual-orientation discrimination. But this research does point out the importance of building safeguards into such laws to protect conservative Christians from aversive effects.

37. We have already seen examples of the link between hostility towards conservative Christians and sexual-orientation antidiscrimination laws. The *Masterpiece* decision turned largely on the anti-Christian comments of the members of the Colorado Civil Rights Commission and how the Commission treated Jack Phillips differently from three other bakers because of his religious objection to creating a wedding cake celebrating a same-sex wedding.

38. Indeed, since *Masterpiece* was decided, Jack Phillips has been sued two more times. In one case, a federal judge in *Masterpiece Cakeshop Incorporated v. Elenis*, 445 F. Supp. 3d 1226 (D. Colo. 2019) found that it was plausibly alleged that the Colorado Civil Rights Commission again demonstrated hostility towards Jack Phillips and may have prosecuted him in bad faith for declining to create a custom cake that violates his religious beliefs. After that case was dismissed, a private litigant sued Jack Phillips again for declining to create that cake. Amended Complaint, *Scardina v. Masterpiece Cakeshop Inc.*, Case No. 2019CV32214 (Colo. Dist. Ct. May 13, 2020).

39. The very framing of the Report can make discrimination against Christians more possible. Barak-Corren's argument is that *Masterpiece* has led to an increase in discrimination against same-sex couples due to religious exemptions from

antidiscrimination laws. But *Masterpiece* did not grant a religious exemption. It was a religious hostility case that found the government acted with hostility. The underlying assumption of the Report's argument is that courts should allow religious hostility against religious wedding vendors to avoid same-sex discrimination.

40. It is also important to explore who is likely to possess anti-Christian hostility. In my previous work, I documented that those with hostility towards conservative Christians are more likely to be well-educated (meaning at least a college degree), wealthy, politically progressive, and non-religious. (Yancey and Williamson 2014, Yancey 2010). These are also fairly accurate descriptions of two groups that have been documented as discriminating against conservative Christians: academics (Yancey 2011, Hyers and Hyers 2008, Gartner 1986) and media personnel (Yancey and Brunson 2018, Kerr 2003). Furthermore, higher education and political progressiveness also predict prejudice against conservative Christians (Yancey and Williamson 2014). To the extent that administrators responsible for enforcing sexual-orientation antidiscrimination laws have college degrees and are politically progressive, then there is reason to believe that such rules would not be enforced in a neutral manner against conservative Christians and in fact may be enforced more rigidly than in other circumstances.

41. If Plaintiffs and those who hold similar beliefs about marriage are not allowed a religious exemption from the Metro Ordinance (and especially if administrators of such laws have a higher propensity to rule against conservative Christians), then Plaintiffs and those with their religious beliefs lose rights to self-expression not denied to other individuals. In this case, the rights of a photographer are at stake. Photographers make artistic decisions as Plaintiffs explain in their Complaint. They can create their photos to express and promote their values. If courts do not protect Plaintiffs' free-speech and free-expression rights, then the

freedom to exercise those rights become tied to the expression of acceptable secular or religious beliefs as determined by the administrators of the Metro Ordinance.

42. The potential cost to Christian communities is possibly higher in the case of an adverse ruling than the potential cost to same-sex couples in the case of a ruling in Plaintiff's favor for other reasons. A positive ruling for Plaintiffs does not require members of the LGBT communities to violate their strongly held norms and values. This is not the case for those in conservative Christian communities. Instead, individuals external to Christian communities (potential consumers and government officials) will assess and approve which values Christians can maintain in their businesses and occupations. I have cited previous court cases and sociological research indicating that those who tend to have anti-Christian sentiment are unlikely to apply the same rules to other social communities that they do to Christians.

43. While some people are tempted to think of religious freedom as issues relegated to only one's thoughts, families, and church/synagogue/mosques, this limited vision underestimates the full value of religious communities and the benefits they provide their members and the rest of society. Religiosity has been found to contribute to an individual's happiness (Childs 2010, Argyle 2003, Stavrova, Fetchenhauer, and Schlösser 2013), reduction of suicide (Ellison, Burr, and McCall 1997, Saiz et al. 2021, Gearing and Lizardi 2009), aid in the coping of trauma (Krause, Pargament, and Ironson 2017, Reiland and Lauterbach 2008), and marital stability/satisfaction (Brown, Orbuch, and Bauermeister 2008, Olson, Goddard, and Marshall 2013, Schramm et al. 2012, Wilmoth, Blaney, and Smith 2015), among other social benefits. These positive characteristics are not simply buttressed by individual's religiosity but also rely upon the social support of a religious community. (Olson, Goddard, and Marshall 2013, Galen, Sharp, and McNulty 2015, Crosby III and Smith 2015, Van Cappellen, Saroglou, and Toth-

Gauthier 2016, McClure 2017). Thus, for individuals to gain the full benefit of their religious belief system, they must have access to a larger religious community. Public-sector efforts that attack this community have the potential to reduce the potential pro-social benefits found within religious communities because these attacks will hinder the ability of those in the community to live out their stated values.

44. Costs of a loss of vitality from religious communities is not limited to those with faith but also to those who may benefit from those religious communities. Given the importance of religious communities in supporting both religious and non-religious charities (Regnerus, Smith, and Sikkink 1998, Brooks 2007), providing support for marginalized groups (Stivers 2011, Sherman 2017, Sullivan 2008), and acting as a buffer against criminality (Jang and Johnson 2001, Salas-Wright, Vaughn, and Maynard 2014, Desmond, Soper, and Kraus 2011, Kerley, Matthews, and Blanchard 2005), it is in the interest of the state to do what it can to aid religious communities. Given that previous research has indicated that highly educated and political progressives are more vulnerable to anti-Christian attitudes and are more likely to enforce antidiscrimination rules against Christian wedding vendors, (Bolce and De Maio 1999, Yancey and Williamson 2014), courts must be careful in how such rules are constructed and be relatively liberal in allowing for religious exemptions.

45. It is possible that an unfavorable ruling against Plaintiffs will inhibit the ability of Christian communities to continue to deliver prosocial benefits to the larger society because it could allow those outside the Christian community to pressure Christians to give up their stated values in order to deliver benefits to that society. In this current case, driving out conservative Christian wedding vendors reduces the services of those business and makes them less assessable to others. The ramifications of such decisions are possibly even greater when considering how

such rulings may eventually help to drive out Christian social work agencies (like foster care and adoption agencies), educational institutions, or perhaps even medical facilities. As suggested in the last paragraph, the loss of the ability of Christians communities to fully serve society will be felt by individuals both inside and outside Christian communities.

V. Conclusion.

46. In closing, Barak-Corren's report is not adequate to predict whether a decision in favor of Plaintiffs in this case will lead to more discrimination against same-sex couples in Louisville. Because of the weaknesses I pointed out, it is reasonable to assume that Barak-Corren has vastly overestimated the potential for discrimination against same-sex couples by wedding vendors in the event of a favorable decision for Plaintiffs. Barak-Corren's study also does not account for the possibility that the policies advocated for by Louisville/Jefferson County Metro Government would have an aversive antireligious effect on wedding vendors. I also anticipate that such policies could over time have a detrimental effect on valuable Christian communities and extract a toll on the larger society.

/s/ George Yancey
George Yancey, Ph.D.

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EXHIBIT A

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Education

1994 Ph.D., Sociology, (1994) University of Texas at Austin
Dissertation Topic: "The Utilization of Weber's Elective Affinity to Reconcile the Macro and Micro Schools within Sociology of Science" (Advisor - Norval Glenn)
1989 M.A., Economics, University of Texas at Austin
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Employment

2019 – Present	Professor, Institute for Studies of Religion/Sociology, Baylor University
2008 – 2019	Professor, Department of Sociology, University of North Texas
2002 – 2008	Associate Professor, Department of Sociology, University of North Texas
1999 – 2001	Assistant Professor, Department of Sociology, University of North Texas
1996-1999	Assistant Professor, Department of Sociology, University of Wisconsin at Whitewater
1993-1996	Visiting Assistant Professor, Division of Social and Policy Science, University of Texas at San Antonio
1994-1996	Special Projects Consultant, Round Top Consulting Associates 11901 Toepperwein Road, San Antonio, Texas 78233

Grants

2020 “Planning Grant for “Evaluation of Effectiveness of Faith Based, and Non-Faith Based Agencies in Creating Long Term Change in Homeless Clients” project. $50,000

2011-2012 “Administrative Support for Development of Christian Studies Initiative” funded by Apgar Foundation. $25,000

2009 “A Qualitative Examination of the Challenges Faced by Hispanic-American, First Generation College Students” $14,695.49

2003-2004 “Handling Cultural Differences Among Interracial Couples” Faculty Research Grant funded by the University of North Texas. $3,500

1999-2001 "Multiracial Congregations and their People" funded by the Lilly Endowment. Co-Investigator with Michael Emerson. $484,884.

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(Paper presentations available upon request)

Community Service

2004-2010 Board of Directors – Mosaix

2002-2008 Campus Advisor – Plumbline (A predominately black campus ministry on the University of North Texas)

2004-2008 Campus Advisor - International Dream & Love Fellowship (A predominately Asian campus ministry on the University of North Texas)

2007-present Campus Advisor – Graduate Christian Student Fellowship.

2007 Consultant – Harrisburg Brethren in Christ. This is a multiracial urban church that sought advice as to how to deal with some current issues of diversity and how to become more racially diverse.

2006 Consultant – Cornerstone Bible Church in Cedar Hill, Texas– This is a predominately white church. I am in the process of using sociological techniques so that I can provide advice that will help it to reach people of other races and become multiracial.

2005 Consultant – Grace Presbyterian Church in Dover, Delaware – This is a predominately white church. I used sociological analysis to evaluate the church to provide advice that will help it to reach people of other races and become multiracial.

1998 Consultant - Bridgebuilders of Janesville/Beloit. (A group of church leaders who are dealing with issues of racial reconciliation). I provided racial sensitivity training for the group.

Non-Academic Publications

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EXHIBIT F

UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION
Case No. 3-19-CV-00851-BJB-CHL

CHELSEY NELSON PHOTOGRAPHY, LLC
and CHELSEY NELSON, PLAINTIFFS

v.

LOUISVILLE/JEFFERSON COUNTY METRO
GOVERNMENT, et al., DEFENDANTS

DEPONENT: PROFESSOR NETTA BARAK-CORREN
DATE: AUGUST 4, 2021

COURT REPORTER: JESSICA TAYLOR ROSS

TAYLOR COURT REPORTING KENTUCKY
2901 SIX MILE LANE
LOUISVILLE, KENTUCKY 40220

PROFESSOR NETTA BARAK-CORREN
AUGUST 4, 2021

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witness in this case?
A. Is that a question?
Q. It is. Would you like me to restate it?
A. Yes.
Q. Does paragraph 12 contain the opinion that you are rendering as an expert witness in this case?
A. Yes, but I would also consider all of the rest as part of the opinion.
Q. So paragraph 12 states based on my work to date, I have formed the opinion that granting Chelsey Nelson a religious exemption from the application of Louisville's Fairness Ordinance in this case could significantly increase a likelihood that same-sex couples attempting to hire wedding vendors in Louisville, Kentucky will experience discrimination resulting in the denial of equal access to goods and services, period.
Is it fair to say that is your top-line opinion in this case?
A. Yes.
Q. Now, you mentioned just a moment ago that you would also point to the paragraphs

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following that opinion as your opinion in this case.
Is that accurate?
A. Yeah. I mean, for me it's stylistic kind of distinction between opinion and basis for my opinion, which is sort of accepted with the format.
Q. Certainly. But in terms of if someone were to ask you as I am now, what is your opinion in this case, paragraph 12 accurately, fairly, and fully summarizes the opinion that you're rendering, whereas the following paragraphs provide the supporting basis for that opinion.
Is that a fair statement?
A. I think so. I think you would say that that's the bottom line.
Q. Now, you note in paragraph 23 of your report that studies like the Masterpiece experiment can never predict future behavior with absolute certainty.
Is that fair?
A. Yes. That's an important line.
Q. I'm sorry?
A. I said that's an important line.

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Q. I agree. So you are not testifying today that you are certain that granting Chelsey Nelson an exemption in this case will increase the likelihood that same-sex couples seeking wedding vendors in the Louisville area will experience discrimination.
Correct?
A. No. There's nothing certain in life or in science.
Q. Are you attaching some sort of probability to the likelihood that such -- such individuals in the Louisville area will experience discrimination 50 percent, 60 percent?
A. No, I cannot attach a specific probability. And what I wrote in paragraph 12 that you cited is that granting Chelsey Nelson a religious exemption from the application of Louisville's Fairness Ordinance in this case could significantly increase the likelihood that same-sex couples attempting to hire wedding vendors will experience discrimination.
Q. And you agree with me that it is possible that granting Chelsey Nelson a religious exemption may not significantly

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increase the likelihood, that same-sex couples attempting to secure wedding services will have experience discrimination in the Louisville area.
Correct?
A. I would say that everything is possible and we have empirical research evidence in order to help us understand what's more possible than not.
Q. But you can't attach a percentage probability to how much more possible it is that couples seeking same-sex wedding services will experience discrimination versus will not?
A. No, I cannot.
Q. Now, in your opinion in paragraph 12, with all the qualifications that you've given, your opinion in paragraph 12, depends entirely on the Masterpiece experiment that you conducted, and that you document in Exhibits 2 and 3 to your report.
Correct?
A. Not only on that, it's also based on my entire research, and the research of others whom I cite my papers. So I would say that the workload between the opinion and the

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Correct?
A. I'm sorry, I'm going to have to ask you to restate because it's getting complicated. Can you rephrase your question?
Q. I will certainly do so. I was actually confusing myself. So to fall within the category negative with referral.
A. Yes.
Q. A vendor would have to respond to the inquiry?
A. Yes.
Q. He would -- the vendor would have to say I will not provide the service.
Correct?
A. I'm waiting for you to continue.
Q. And the vendor would have to make a referral to another name?
A. That's right.
Q. Okay. No other criteria were assigned?
A. Either referral or as you've seen, I mean if they were saying no, I can do that, but I can give you something else.
Q. Okay. And so a -- and when you say that something else, would that have been

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something like a non-custom cake?
A. No. So --
Q. Okay. What -- what falls in these, something else?
A. So for example, if you were asking for -- I was thinking originally that -- that you would -- you could find there. No, I was doing this research on Masterpiece and I was thinking on Jack Phillips's argument that he was willing to provide cupcakes and not wedding cakes. But as you see, that turned out to be a very small category of cases.
Q. Yes. And when you were -- and I imagine you probably looked at some of the raw data, when you were looking at the referrals or you were looking at the responses ultimately landed in this column, negative with referral --
A. Yeah.
Q. -- did you see any instances of the vendor offering to provide services other than those that were requested by the auditor?
A. There were very few of those. Very few.
Q. And in those instances, what types of alternate services were being suggested?

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A. Sometimes people say, we cannot do -- we cannot do -- we cannot photograph the event. But, you know, mostly they would say, you know, maybe you could turn to someone else or they would say, I do not offer these services anymore, but -- but I do other things. You know, maybe people have switched from photography to videography or something or these kind of stuff. And we were coding them consistently because sometimes people said that to one person but said something different to another person. So the fact that someone said that they are no longer doing photography, for example, does not mean that they would say that to the -- to another couple that would -- that would contact them.
Q. Did you encounter any vendors who responded to you and said, I provide services of this type, whether it be bakery or photographer, but I simply do not provide services for weddings?
A. The funniest thing we did -- I don't remember exactly whether we had something so specific, but we did have someone who said, you know, I don't -- I no longer -- I'm no

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longer in the business of photography, I am going to become an astronaut. So that person ultimately was excluded from the sample because he responded the same thing to all. So, you know, we realized he wasn't supposed to be there.
Q. Yes. That's that very common photographer to astronaut career path.
A. Right. I thought you would appreciate that.
Q. Yes. And that's -- I would expect to hear that answer more often. So --
A. I'm trying to do my best.
Q. So then you have a category in the middle there. I'm looking again at table OA4.4. It says, no response. And I assume that just means, as your report says, as your documents say, the vendor simply did not respond to the inquiry.
Correct?
A. Yes.
Q. Now, my understanding, and correct me if I'm wrong, if a vendor reached out after the seven days or a period after the audit questions were provided, after the e-mail was

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sent, if the vendor reached out late, like 8, 9, 10, 11 days after they would either fall in the no response category or be tossed out of the survey? I can't recall which it was.

A. No. They would be coded.

Q. Oh, they would be coded. Okay. So even if they responded well after -- afterwards, they --

A. I never -- I never code or analyze data before everything is complete. It's a bad practice.

Q. So in order to fall within the no response column, a vendor simply had to not respond to the e-mail that was the subject of that particular wave?

A. Yep. And I want to -- I mean, I don't know if it's important for your purposes, but I want to be clear that we were not coding automatic responses as responses. So it had to be an active response.

Q. Yes. Understood. Like a -- like an automatically generated response from the Microsoft Outlook program, saying --

A. E-mail, I'll return to your later, right.

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Q. That was not a response. Okay.

Positive responses, when we mentioned that it was cooperative, describe for me what -- what kinds of specific responses fell within that cooperative column?

A. So I think I talk about this in the -- in the nuanced part of the appendix. So I'm just going to read from there and I'm happy to expand if you'd like me to. So for example, they could be asking for additional information about the location of the venue which was not specified in the e-mail. And that strike me, as at least initial willingness. So it's not a negative. So -- but it wasn't a full positive. So that -- that got 0.5.

So sometimes it looked like they were willing to consider, but they need more information. And even though every e-mail received a response, then to not taint the study with our responses then we didn't consider what comes after our -- so let's say they asked us, you know, about the venue and, you know, then I give -- I give them a -- I give them a -- a village that's nearby their business, and then they say wonderful, great. I wouldn't correct

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the coding because my answer maybe interfered with their judgment. So they remain 0.5.

Q. And then positive was obviously an agreement to provide the service on the date given?

A. Yeah, like, wonderful, we'd like to work with you. What did you have in mind?

Q. Okay. Now, going back to paragraph 13 of your report in this case.

A. Yep, I'm -- I'm looking at it.

Q. Good. You talked about the wedding market?

A. Yes. I see that.

Q. Yes. You said you were trying to get a -- you were examining the impact of the Masterpiece Cakeshop decision on the wedding market. And I assume that you were now in this report attempting to evaluate the impact of a potential decision in Chelsey Nelson's case on the wedding market in Louisville, Kentucky.

Is that a fair inference?

A. Yes.

Q. And the wedding market, as I understand it, is the collection of vendors who provide wedding-related services to residents of

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a particular geographical jurisdiction.

Correct?

A. Yeah. I mean, it could be whatever jurisdiction or a geographical area you have in mind. And of course, you know, there could be -- Louisville is -- is the center of Kentucky. I mean, then it's part of a metropolitan. I mean, it could have additional influences as well, but yes.

Q. Yes. And is it fair to say -- I'm looking at page 4 of Exhibit 3 to your report, which was the draft article for the Journal of Legal Studies. At the bottom of Page 4, you listed about ten -- you list ten types of vendors who are participants in the process of organizing a wedding: Reception venues, wedding planners, bakers, florists, photographers, videographers, bridal and groom salons, jewelers, DJs, and calligraphers.

Do you see that?

A. Yes, I do.

Q. Is it fair to say that is -- that that collection of vendors constitutes the wedding market for your purposes?

A. Well, I mean, there are lots of

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people providing services for weddings and I would say that these are among them.
Q. Yes. Are there others that you have in mind besides these ten?
A. These are the -- these are, I think, the most common ones. Of course, couples could use more or less of them.
Q. All right. So and you agree that not every couple takes advantage of a DJ for -- for example?
A. That's right. But bakers, florists, and photographers are probably the most -- the busiest ones, almost in every wedding, you have them. If not all -- all weddings.
Q. Reception venues are pretty common as well?
A. Right. Although, I mean, couples can also marry at their parents' home. I mean, that depends that there are lots of -- it's a very complex legal transaction to organize a wedding --
Q. Right.
A. Lots of serious transactions.
Q. Right. One that I see missing here

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will be the wedding venue, for instance, assuming it's different than the reception?
A. Okay. I meant -- I meant the venue. You could add that as well.
Q. I see. Okay. Now, did you do anything to evaluate whether the attitudes and the willingness of each of these various members of the wedding market are identical with respect to your willingness to serve same-sex weddings?
A. No. We don't have any information on that, I think, anywhere.
Q. So you can't tell me whether a wedding venue provider is more or less likely to agree to provide services to a same-sex wedding than a baker?
A. So what I can tell you based on my study is that I looked at these differences. I also write about it and I found no differences in the specific sense that we're interested in, which is whether those different business categories would respond differently to same-sex couples following Masterpiece Cakeshop, the decision.
Q. But you didn't study reception venues?

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A. No, I didn't.
Q. You didn't study -- you didn't study videographers?
A. No. Although they might be similar to photographers, but I didn't study this group, no.
Q. You didn't study wedding planners?
A. No.
Q. You didn't study jewelers?
A. No.
Q. You didn't study DJs?
A. No.
Q. You did not study calligraphers?
A. No, I did not.
Q. You did not study bride and groom salons?
A. That's correct.
Q. Now, did you also study whether same-sex couples are more or less likely to select wedding service providers based on referrals, or are more likely to simply cold call or pick them out of a phonebook?
A. So that's an interesting question. That brings me to my background research on the wedding market, which I did in order to begin

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this study. So it seemed fairly common for -- just based on researching what couples do online. And you can see some of this in -- in my studies in the references, but I did a lot of -- I -- you asked me about my field of expertise before. I should -- I forgot to say that I'm also an expert in planning a wedding after this research.
And what I found was that it's very common for couples to just search online in, you know, in the surroundings of the place where they want to marry and look for business -- businesses, vendors who would be interested in. And there are also websites. For example, The Knot that just vendors can register there and then you can search there for wedding vendors very conveniently and, you know, based on area and -- and all kinds of stuff. And The Knot is not the only one. So and, of course, I mean, referrals are also an option. But it seems very common for -- for a couple to just search online for -- for vendors.
Q. Now, when you say you became an expert wedding planning, are you referring to -- to your own wedding?

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A. My own wedding was before this study.
Q. Okay. I wasn't sure if you were making a reference to that. So but did you -- I hear what you're saying, but did you do anything in terms of quantitative -- or sure qualitative or quantitative analysis to determine how frequently a same-sex couple relies on referrals versus how frequently they rely on simply choosing vendors unknown to them previous out of the phonebook or off of -- off of an online resource?
A. No, there is no information on that available to analyze.
Q. Sure.
A. And I was doing my research on what couples do bases on what's -- what the resource is available for them. Yeah.
Q. Sure. You would agree with me though, I mean, the wedding market varies from state to state, geographical location to geographical location.
Correct?
When you found that, for instance, Iowa had apparently a shortage of bakers and you

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had to turn to florists to supplement your research in Iowa.
Correct?
A. That's right.
Q. All right. And so it's reasonable to believe that the ways in which couples identify and locate wedding vendors will vary from place to place. Wouldn't you agree with me that that's a reasonable assumption?
A. No, I don't know that. I mean --
Q. You don't know what -- you don't know either way?
A. No. I mean, if we're looking at the examples from the case law, then we can see that conflicts can occur either way. So for example, the couple that entered Jack Phillips's store came there because they got a recommendation on Phillips's cakes from their wedding planner. They didn't know him before.
And then with Arlene Flowers, that was a long-time -- a long-time customer. But then with some other cases like the Aloha B&B hotel then that was like a cold call in your terms. And you know, there are some cases also, I mean, in many, you know, I -- I could also go

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outside of the United States and -- because these cases are recurring all over the world, but there are cases of both types.
Q. So you're not telling me though that you're relying on case law to inform your views as to how couples identify wedding vendors, you're not -- that's not what you're telling me.
Correct?
A. No, I'm just giving you an -- so expanding your views on how kind -- how kind of -- how same-sex couples might come to engage with wedding vendors.
Q. Sure. That you would agree case law is actually highly unrepresentative sample of how interactions and conflicts between religion and principles --
A. I would -- I would think you may be citing my research now.
Q. I think I'm thinking of your Villanova Law Review article.
A. That's correct.
MR. BANGERT: Yes. By the way, I am offering to our reporter, are you in need of a break at this time?

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THE COURT REPORTER: I'm okay.
BY MR. BANGERT:
Q. Good. Okay. Let's talk about -- the Masterpiece decision was something that factored heavily in your research.
Correct?
A. The Masterpiece decision itself?
Q. Yes.
A. Of course.
Q. Of course. Now, the Masterpiece ruling did not yield a religious exemption for Jack Phillips, right?
A. I think that depends on how you look at it. That's actually disputed by legal scholars.
Q. Right. The -- the upshot of that decision was the Supreme Court found that the Colorado Commission acted with hostility towards Jack's religion and for that reason had violated the free exercise clause.
Correct?
A. Yes. But then afterwards, the Supreme Court remanded two other cases where there was no allegation of hostility on the record, you know better than I do, and returned

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them to the lower courts.

And I should also say, I mean, that Masterpiece was more vague than clear about the exact basis for the ruling. And that allowed the decision to be portrayed, even by Jack Phillips himself and his lawyers, as a victory for religious freedom, which is not just about the proceeding in Colorado. I mean, Phillips is quoted saying -- I can find that if you'd like -- that, you know, from now on every religious person can feel free to follow their business and to run their business according to their values. So obviously, there is what the court does and what people understand the court to do. And --

Q. Yes. I -- I agree.

A. And even legal scholars can disagree about the interpretation of certain court decisions, which is what happened in this case as well.

Q. Well, there would be no law review industry without such disagreements. Now, you would -- do agree with me though, the court, because it detected religious hostility in the Colorado Commission's decision, that it did not

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conduct a full-blown, full scrutiny -- strict scrutiny analysis of the Commission's actions but more or less assumed it was a violation per se?

A. I would agree with this first part of what you said that he did not conduct a full strict scrutiny analysis, at least not the majority opinion. Of course, we have the minority.

Q. Correct. Correct. Now, your -- your analysis of the Masterpiece opinion's effect on the willingness of vendors to supply services to same-sex couples really depends upon their -- depends upon those vendors reaching some understanding about the content of the decision that was rendered by the Supreme Court.

Is that fair?

A. Either by themselves or by an atmosphere that was surround -- that was -- that evolved out of the decision.

Q. Of course, in ways that that can happen. The vendor themselves could read the opinion. The vendor could read media accounts of the opinion, or the vendor could have personal conversations with others, whether by

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in-person or on social media or online about the decision, but any of those would be a way --

A. And they can also listen to the radio or, you know, watch TV, et cetera, et cetera.

Q. Sure. I would -- I would classify all those as media-driven accounts of the content of the decision.

A. Yes.

Q. But fundamental to your opinion is the assumption that vendors will become aware, one way or another, of the content of the Court decision that you would -- you would argue is driving their behavior or influencing their behavior?

A. Well, research -- some research -- research seems to show that they don't have to actually become aware of it. So imagine the -- I don't know if that's of service to you but imagine the following scenario. Say that you and I are both wedding vendors or maybe that you're, you know that you're a friend of mine, okay? And you read about this decision and I'm a wedding vendor and, you know, you come to me and you don't tell me about the decision, but I

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get from everything that you're saying that you're in favor of, you know, for example, denying service to same-sex couples to interracial couple to whatever it is. I might be influenced by that, even if you don't tell me that anything has changed.

So sometimes court decisions influence how people behave or what they think through their shaping the -- their perceptions of the social norm. So we have several studies that shaped -- that showed that this is what happens with Supreme Court decisions.

So I can talk about one. So for example, Obergefell. So with Obergefell, two social psychologists from Princeton, Betsy Levy Paluck and Margaret Tankard, have studied over a series of national opinion surveys, et cetera, that they started with before Obergefell and then followed with after Obergefell a month -- for a month after the decision.

And they studied both people's personal views and their perceptions of the social norm towards same-sex marriages. And what they found was that the people's personal views were not changed, but people's perceptions

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of the social norm regarding the legitimacy of same-sex marriage have changed after the decision. And they also found that this was not affected by the frequency of media consumption. So the -- the -- their finding suggests that there could be other ways, either -- other than consuming media about the decision where message of the court actions spread in society and shape social views.

Q. You're an attorney, so you're -- you're familiar with the concept of attenuated causation, proximate cause, and but for causation, the rules governing causation, and it strikes me, you were -- you were attempting to measure the direct actual cause and effect relationship between the Masterpiece decision and decisions by vendors whether or not to provide service to same-sex weddings.

That was your objective?

A. Right. And since -- okay. Let's -- let's -- let -- let you finish your question. I would not want to preempt your question.

Q. No, I was -- I was just trying to confirm that was your objective.

Did you do any work to evaluate

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whether the vendors you contacted as part of your Masterpiece experiment had any knowledge of, appreciation for, or understanding of the Masterpiece decision after it was rendered?

A. No, I couldn't do that. I mean, it wasn't a survey. You asked me before how I'd like to call it. I was contacting them -- contactin them as discerned customers wishing to get service. They're -- these wedding vendors are not -- they're not part of any -- of any survey, company panel or something.

So I would say that they -- there were several studies conducted in other Supreme Court decisions before mine that measured attitudinal changes, asked, recruited survey participants and some of them collected information on media consumption. And I can talk about what kind of met -- lessons we learned from those studies.

Q. Okay. I don't want to ask you that question, but I do want to ask you this: Are the studies that you're talking about right now mentioned anywhere in your expert report or the exhibits thereto?

A. Yeah.

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Q. Okay. Can you point me to where in your report, these studies that you're talking about right now are mentioned?

A. Do you have it printed or do you want to search it?

Q. I have -- I can search it. I can do either.

A. So the main study that we can talk about, because I think it did the best job, is the study of Katerina Linos and Kimberly Twist from 2016. And if you're looking at the JLS piece where I'm at, but we can look outside at the other piece, I think it's mentioned there as well. So it's mentioned on page 10 and 8 and twice on page 10, 8, and in page 42 in the references.

Q. Okay. I don't need to discuss that article so much as just make sure I knew where to find it. Any other references that would explicate this idea that you were just telling me that -- that knowledge of a decision by a court can filter into the public consciousness, even without a member of the public consuming media about the decision or reading the decision itself.

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A. Sorry, I just -- before I mentioned Linos and Twist, I mentioned Tankard and Paluck and they're mentioned on page 44 of my references going by Tankard.

Q. Okay. And any other references that you would point me to for that proposition?

A. And there was also another, the -- I cite additional two studies that measured the impact of the legalization of gay marriage on public opinion. They're Kazyak and Stange from 2018 and Ofosu, O-F-O-S-U, et al., from 2019. These are all studies that are directly relevant to this project and -- but they're also more, I mean --

Q. Now let me ask about one cited in your -- cited in your report.

A. Okay.

Q. So is that -- have we covered the waterfront of sources that would support the proposition we were just discussing that are cited in your report?

A. Yes.

Q. Thank you. Now, you mentioned -- I want to go back to -- I want to change gears a little bit and talk about a slightly different

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match my -- the -- the -- the sort of religious environments where my businesses were located with -- with their data and have that data inside my data set.

Q. Yes. And so when I -- when I -- when I said that you devised the measure, you were relying -- I understood that you were relying on publicly available data. But in terms of the concept of religious density, you weren't able to go to a publication and say, ah-ha, the religious density of X metropolitan area is this. You had to make it -- you had to perform a calculation on your own using publicly available data in order to arrive at a measure of religious density.

That's -- is that fair?

A. Yeah. But both the population and the number of congregations were taken from the same source. So that --

Q. Yes.

A. -- available data. Yeah.

Q. Yes.

A. So that I was really just normalizing the number of congregations with the number of people residing in the area.

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Q. Yes. Yes. Now, something that I didn't see in your report and I'm curious, did you look at the size of the congregations such that some areas -- I'm sorry.

A. Let me -- yes. So I didn't and the reason why I didn't is that -- on that -- on the -- the surv- -- the very large survey that I'm referring to, they explicitly say, the researchers, that this is not reliable. So they collected information both on the number of congregations and on the number of congregants. But they explicitly said that they think this is not reliable. They had several reasons to think that.

First, there were some congregations that reported on more people than the people in the county. And there were some mismatches with the numbering of other congregations and they just felt that it's not reliable. So they explicitly, sort of, warn researchers about this measure. So, you know, I wanted to -- to play safe and I just looked at the number of congregations.

Q. Of course. So -- so we have this concept of religious density that we've just

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discussed.

A. Yes.

Q. Setting religious density off to the side, over there, apart from that, there was no other measure that you could look at to determine the religiosity of a particular metropolitan area.

Correct?

A. Not at the county level, not that I was aware of.

Q. All right. So -- so you really had so the -- the density measure is really what you had to look at when you were trying to evaluate the religious context at the county level in which a particular wedding vendor exists or lives in?

A. That's right.

Q. Now, did you do anything to evaluate whether the wedding vendors who were contacted as part of your audit, understood or knew about the legal regime in which they operate? When I say legal regime, I'm talking specifically about whether it's a RFRA or non-RFRA state and whether or not there's an applicable anti-discrimination law in their

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particular jurisdiction?

A. No, I don't. And I actually write about that in the paper, so you must have read that. But it's actually very difficult to make these kinds of inferences. So there is a host of research in the field of law in society. You know, what shapes what and whether people know or do not know on the content of laws where they're at. So it would be too strong an assumption to think that people are necessarily are aware. Although you might think that businesses are more aware than just, you know, regular individuals about the content of laws that are relevant to them. But still, I mean, it's -- it's too strong an assumption. And for the same reasons that I couldn't survey that -- those businesses that weren't survey respondents about their media consumption habits, then I didn't survey them about whether they know the state of the law.

Q. Right. And I think that's probably why you said in your report that you're not arguing for a causal relationship between the specific legal regime within which a vendor operates, and that vendor's particular attitudes

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Q. You do. Okay. So --
A. I mean, I would say that I -- I haven't been researching this field in 2020 and 2021. But back then in 2018, I was a real expert on planning weddings in the U.S.
Q. Okay, but in terms of I guess --
A. I wouldn't plan your wedding now, maybe the field has changed.
Q. I see. So you do not hold yourself out as an expert in wedding planning today in 2021?
A. Right. I don't know what COVID did to weddings.
Q. Fair enough. I don't think it did anything because no one's getting married right now, apparently. Well, actually, I've had several friends get married, so that's not true. It's an unfair statement. Okay.
So what -- were there specific sources that you looked to for this information that your report in footnote 21?
A. So I see footnote 21 doesn't report sources, but other footnotes report sources of exemplar websites and wedding manuals. So that was -- that was something I researched quite

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heavily.
Q. Okay. And all of the research that you conducted into the wedding market is referenced in your report, right? At least all the -- all the resources that you're relying upon to inform your expertise in the wedding market?
A. Not necessarily. I mean, journals often want you to cut words from footnotes and cuts resources sometimes. And so things that they didn't see material may have been dropped. But enough sources are there in terms of, you know, types of websites and so on, to give you a sense of the sources that I was using.
Q. Are there any sources that you can think of right now that directly inform the information that you convey in footnote 21 that are not reported in your -- in your report?
A. I don't know. I mean, maybe there are more because I always choose -- I mean, there were so much -- so many of them that maybe I chose just several for the footnotes that were sort of referencing the kind of wedding manuals that I was using. And so maybe there are more than that, right? But I mean, if you're asking

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me right now, I would say that I don't know. But I could, I mean, Casey said I shouldn't offer to look for stuff, but --
Q. Well, I will comment -- I won't comment on the advice of counsel. I'm not asking you about the advice of counsel.
Let me ask you this, let's go to your -- let's go to your Online Appendix, which is Exhibit 2 to your deposition.
A. Sure.
Q. And in your Online Appendix, you report the script, I won't call it a script, but the text of the e-mails that you sent --
A. Sure.
Q. -- to vendors as part of the various waves of your report -- of your study.
Is that correct?
A. That's correct.
Q. There are all of the various -- there are all of the e-mail texts reported here in your online account?
A. Yes.
Q. Now, who drafted the text of these e-mail scripts?
A. I did together with my research

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assistant. And we also had someone who's native English speaker to go over it -- more than one, actually.
Q. And the reason I asked that question is I noticed the -- the script that was used for waves 1 and wave 2 -- wave 1 and wave 2 --
A. Yeah.
Q. -- was very similar. But when you get to the script for waves 3, and again, for wave 4, I'm going to describe the language as much more colloquial in nature. For instance, I see -- I even see a, woo-hoo, in one of them, the photographer e-mail for wave 3.
A. That's right. There was a, woo-hoo there.
Q. Yes.
A. But we had something similar to a woo-hoo, I think, in one of the 1 and 2 e-mails as well. But in any event, you're right to note that there was a change in the style or tone of the e-mails from before and after Masterpiece and that was deliberate.
Q. Okay. Why was there a change in style and tone after Masterpiece?

affirmative.)
Q. -- most of those positive responses landed in the no response category, correct? Most of the decrease.
A. That not -- not necessarily the case. So if you ask -- if you're looking at within subject, so if you're looking at the same subject over time, then we need to look at a different table. If you are interested to know across all subjects what happened, then you can see the data here. There's also an increase in negative, like explicitly negative response.
But it's true that across all subjects, you see that the main form of providing a negative response is to provide a no response. So to not provide a response at all. But when you are looking at within business transitions, then you see something a little bit different. So we can -- we can go to look at this table if you're interested.
Q. And I am, but let me just, kind of, finish my thought on this. You mentioned the -- the no negative responses just a moment ago, the actual negative responses. And it's true that negative responses to inquiries into same-sex

weddings increased from 4.0 percent prior to Masterpiece to 6.3 percent after Masterpiece. I'm looking again at the table OA4.4.
Do you see that?
A. Right. Or you can look -- or you can look as I look below table OA4.4 at combined negative responses. So they increase from 10.28 percent negative response before Masterpiece to 16.5 percent negative responses after Masterpiece.
Q. And that's -- that's a different manipulation of the data here. But I -- I'm also interested by the fact that negative responses to opposite-sex inquiries also increased --
A. Slightly, yes.
Q. -- over that same time period. Well, if you look at the opp- -- the negative responses to same-sex inquiries went up 2.3 points.
A. From -- from about 10 percent to about 13 percent. So about half the -- half the growth of same-sex couples and that's statistically significant.
Q. And again, I'm looking -- and again

I'm only looking at the data reported in this table, not necessarily how it's normalized below, but I'm looking at -- for opposite-sex couples, negative response rates, actual negative response rates, actual nos, we talked about this earlier. Actual nos, I'm not going to do your wedding, went from 2.9 to 4.8, which is an increase of 1.9 points.
Correct?
A. So again, both types of no are nos. The question of whether there comes another politeness after the no is I call this as a negative with a referral but it's still a no.
Q. Right. And -- so let's get to --
A. Still an actual no in your terms.
Q. Yes, a negative with referral, there was an increase from same -- same-sex couples of 0.1 point and for opposite-sex at 1.2 points.
A. Uh-huh. (Witness answers in the affirmative.) What you're looking at just so make sure --
Q. I'm looking at negative -- negative with referral.
A. Negative with referral and would

you repeat the numbers?
Q. Sure. For opposite-sex, actual negative responses with referrals went from 2.8 to 4.0 percentage points.
A. Uh-huh. (Witness answers in the affirmative.)
Q. Whereas for same-sex couples, 3.1 to 3.4 percentage points.
A. Uh-huh. Yep.
Q. If you combine the increase, the actual negative responses to opposite-sex couples increased more percentage points than actual negative responses to same-sex couples from pre to post-Masterpiece.
A. Only if you're looking only at negative with the referrals. But if you're looking at the negative responses together, you see the opposite picture. Which is that the percentage of negative responses that same-sex couples received increased 177 percent. The increase of such responses for opposite-sex couples. But as -- again, as I say below the table, these are small numbers. I wouldn't base any, you know, statistical inference only on those numbers.

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Q. Okay. Let me stop for a moment to make sure when you're saying those numbers, we're referring specifically to the increases that we -- the -- the -- the changes that we've been discussing in actual negative responses or actual negative responses with referrals, those are small numbers is what you're saying?
A. So what I'm saying is that each of those in separate are very small numbers. And even when you combine them together, these are not a lot of responses. But when you combine them together, it does make more sense to try and do the exercise that you're doing.
Q. Right. But even when you combine them together --
A. And that -- and that exercise is reported below the table.
Q. Yes. And I -- and I have seen that report where you note that you went from -- you basically increased a raw total of 22 additional negative responses from 66 to 88.
Correct?
A. Why -- and that's why I said the numbers are small. But I think that we should look at positive responses, but I'm consistent

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in treating together negative responses and positive responses in both kinds of analysis. So both -- both the cooperative and the positive are analyzed together and the negatives and negatives with referrals are analyzed together. And the reason is really simple because the numbers are so, so small when they're looked at apart.
Q. Yes. And I -- I want to -- there's something that I think is constructive here, you -- we talked about this earlier in your deposition, but one of the headlines -- one of the headline findings that you identified as part of your report was that willingness by wedding vendors to serve same-sex couples fell from 63.6 percent positive response rate to 49.2 percent positive response rate, pre and post-Masterpiece.
Correct? Recall, we talked about that?
A. Right. It's there in the introduction where I summarized the findings.
Q. Absolutely. And we've been talking about -- we've been, sort of, going back-and-forth about the columns in table OA4.4

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that talk about negative and negative with referral and I think we all agree, those are as you said, small numbers?
A. Uh-huh. (Witness answers in the affirmative.)
Q. The driver, the thing that drives the fall in positive response rates to same-sex wedding inquiries, pre-Masterpiece and post-Masterpiece, is largely the increase in the non-response rate to same-sex wedding inquiries, pre-Masterpiece and post-Masterpiece.
Correct?
A. Right. Will I get to explain anytime soon?
Q. Of course. I just -- I want to make sure I understand something about that. And you did code non-response as a -- as a negative response.
Correct?
A. I did.
Q. Yes. And you agree it's possible that a non-response from the vendor could have been because the vendor was too busy to offer a service requested, right?
A. Absolutely.

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Q. And you agree that a non-response could have meant the vendor was no longer offering the wedding services requested?
A. Unless they were trying to be astronauts.
Q. Unless they were trying to be astronauts. It's possible that a non-response from a vendor means the vendor was already booked for the dates requested?
A. Absolutely.
Q. The same can mean the vendor just simply is bad at checking his or her e-mail?
A. Absolutely.
Q. It could mean that the vendor doesn't know how to use his or her computer?
A. Absolutely. But what you're missing is an important assumption.
Q. Okay. Before I get to that assumption though, you mean, the vendor could have been on vacation when the e-mail arrived, right?
A. Any number of reasons. And that's why we never -- never infer anything particular about a single vendor from a non -- no response. We only look for in these kinds of studies,

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that there was tremendous attrition between waves 1 and wave 2. You've noted that several times in your report, and that certainly complicated your ability to analyze changing attitudes post-Masterpiece. Do you agree with me though that in order to evaluate whether Masterpiece, the Masterpiece decision was the causative effect -- and I'm -- I'm going to set this question up this way.

You have a business prior to Masterpiece agreeing to serve or gave you a positive response to a same-sex wedding inquiry.

A. Uh-huh. (Witness answers in the affirmative.)

Q. After Masterpiece, that same business gives a negative response or no response to a same-sex inquiry. So this is one of those businesses that you've been describing as it changed it's mind, so to speak. It went from a 1 on the binary scale to a 0 on the binary scale, right? And you have the Masterpiece Cakeshop decision intervening between those two positions. You would agree with me that it's important in order to isolate the Masterpiece Cakeshop decision as the

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causative agent for that change, that you would need to be asking that business virtually the same question, both before and after Masterpiece?

A. No, I would not agree with you.

Q. Okay.

A. And -- and that's -- that's my -- that goes back to my explanation from before. Okay. So what I'm doing is I'm not looking only at the change from, did you serve same-sex couples before to would you serve same-sex couples now. If I would do that, my estimates from discrimination would have been much larger, but I think less precise. Exactly because of the kind of line of questions that you were concerned about of the changes that occurred from before to after Masterpiece. So this is not what I'm doing.

What I'm doing is I'm looking at all of the businesses that prior to Masterpiece were willing to provide service to same-sex couples. And what I'm doing now is I'm comparing how they behave to same-sex versus opposite-sex couples after Masterpiece. So I'm not looking at only, at what -- how they changed

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with respect to same-sex couples. I'm just looking at how they behave to -- to identical inquiries from opposite-sex and same-sex couples after the decision is rendered. And that's crucial.

Q. Right. But -- but your attrition rate in wave 2 really compromises your ability to understand how those same businesses reacted to both opposite-sex and same-sex couples together prior to Masterpiece. Because you've said that many times.

A. Right. So I concede in the paper that I'm not able to know what is the extent of discrimination towards same-sex couples if there is any before Masterpiece. So that I'm not able to know. But by focusing on the businesses that were willing to serve and seeing whether they distinguish between couples of both types after, and these were all businesses that were 100 percent willing to serve same-sex couples before.

And by the way, I mean, going back to your first line of questions on this, the e-mails that they received afterwards were more business-friendly than the e-mails they received

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before. They were more precise, they were more action-oriented, they were more specific in terms of date. They -- the profiles had pictures. Lots of things changed for the better. Yet, they were dis -- discriminating now against same-sex couples, which they didn't do before just based on the fact that they were willing to serve them before.

Q. Okay. And now, you would agree with me though, that if you introduced in the e-mail communication before Masterpiece versus after, if you introduced a new variable in the e-mail communication after Masterpiece to a vendor that previously was willing to serve the same-sex couple, that that new variable that you introduced could contaminate your results and make them less reliable in measuring the impact of the Masterpiece decision on that vendors willingness to serve?

A. No, only if I would have introduced the variable differentially to both couple types, but I didn't do that.

Q. Okay. Well, I -- and that's where I want to focus for just a moment in your Online Appendix.

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Q. Uh-huh. And it's also fair to say that if a wedding vendor declines to provide service or provides no response to an e-mail, requesting service on a date for which the vendor is booked already, that the decline or the non-response has nothing to do with animus or discrimination based on sexual orientation?
A. Sure.
Q. And --
A. But again as you scroll, I want to explain.
Q. Okay, then.
A. Okay.
Q. You can explain. I just want -- there's no question pending right now.
A. Okay. So let me -- let me explain why this change is actually necessary to implement substantive equality between waves 1 and 2 and waves 3 and 4. So the timing of the prospective wedding that was noted in the e-mail was determined uniformly within wave and for each couple based on the same market norms. So when you were contacting vendors a month afterwards, if you give them exactly the same day that you gave them a month before that, you

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actually give them less time ahead. So you're giving them less than a fair chance of being able to provide you the service.
So whenever you contact vendors, if you want to keep being within the market norm, of contact them, say 11 months ahead -- away for photographers and eight months away for a bakers and florists, you need to change the date. So that's the first thing. Why the date had to be changed after Masterpiece. It would have been necessary either way.
In addition, I moved from specifying a month to specifying a weekend or a date specifically. And the reason for that was, as I noted before, that I need to create some change between e-mails 1 and 2 and e-mails 3 and 4 that would make them seem sufficiently different so as not to arise e-mail fatigue, suspicion, or all of the other kinds of things that I didn't want to raise. And giving a specific weekend, which again was identical substantively between the two couples, same-sex couple and opposite sex couple after Masterpiece allowed me to achieve that. Now you were asking about how I coded those responses. So I coded

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that --
Q. I wasn't asking you about that actually.
A. Okay. So I won't answer.
Q. Okay. I do want to know one thing though. You would agree with me that it's more likely you're going to get and -- it's more likely that a wedding photographer is going to be unable to provide service if you only give that person one date to provide the service versus 30 or 31 dates?
A. But I did that for both same-sex and opposite-sex couples. So whatever unavailability patterns you'd should -- you'd seen, they should have distributed equally across couple types unless there is discrimination.
Q. And for waves 3 and waves 4?
A. Yes, that's correct.
Q. Yes. And waves 3 and waves 4 both were sent out after Masterpiece decision was rendered?
A. That's correct.
Q. Right. But it was certainly more likely that when wedding vendors contacted prior

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to Masterpiece, would have had more availability in response to the e-mails that were sent because you gave them more dates to choose from?
A. That's possible. But that means that my group is just larger and I'm, you know, more conservative in my test.
Q. Right. But you -- you just don't know how to compare the response rates to same and opposite-sex couples pre-Masterpiece because you had such tremendous attrition from waves 1 and wave 2?
A. So again, what I'm saying in the paper is that I'm not attempting to compare them and to discern anything about discrimination before Masterpiece.
Q. Right.
A. My objective is -- my objective from the beginning, was to measure whether Masterpiece changed anything. So I was obv- -- and obviously, I would have wanted to be able to also say something about what happened before Masterpiece because there is really no data on that before.
Q. Right.
A. But my main objective --

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Q. So you mainly can say --
A. But my main objective was to understand whether there was change. And whether there was change that I can still and do examine and record.
Q. Certainly there was change. But at the same -- you know, I'll leave it there.
A. But I want to emphasize my coding policy was identical. So if someone offered a date that -- that was outside of the month, in waves 1 and 2, they were coded exactly the same as if they would've code -- asked for a different wet -- weekend in waves 3 and 4.
Q. Right. So if the -- if the wedding vendor in wave 1 said, I can't do it in January but I can do it in February.
A. Yeah. That would have been the same. Yeah.
Q. Sure. Is it significant to your analysis that the Texas RFRA exempts local anti-discrimination laws?
A. It is. I would have written a different --
Q. And why is that?
A. -- I would have written a different

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report otherwise.
Q. I see. My understanding about the Louisville mark- -- is it that the -- this kind of gets into a critical question that I want to ask you. And -- you can take a look here right now. And I'm going to -- to do something that lawyers are never supposed to do, but I'm going to do it as a courtesy and that is to tell you I'm about 65 percent through my deposition, possibly 70. So I say that to you for timing purposes because I know it is late there. So I certainly would, you know, be willing to keep going and try to wrap this thing up this evening or we can come back for maybe an hour or two tomorrow. I'll -- I'll give -- I'll -- I'll make it your choice. I just -- I was noticing that it is probably 11:36 your time, right?
A. That's correct.
Q. So --
A. So I -- I think probably the university locked me in here in any event and I'm still up. So let's try to go on until it's enough for today.
Q. Okay.
A. I'm not sure that I can get out if

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I wanted to. That's what I'm saying.
Q. Cool. I -- I rarely have the opportunity to have a witness who's locked in place. And so this might be one of those --
A. Your opportunity.
Q. This is my opportunity apparently.
So something that was critical I think to your report was, trying to draw a connection between Louisville, Kentucky and the four states that were studied in the Masterpiece experiment.
Is that fair?
A. Yeah.
Q. Okay. And I know we've already covered this, but just to reset ourselves, you did not actually study wedding vendors in the state of Kentucky?
A. That's correct.
Q. As part of the Masterpiece experiment?
A. That's correct.
Q. Nor did you conduct a follow-on study for purposes of this report?
A. That's correct.
Q. Sorry. I'm getting e-mails from --

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I can -- okay. It looks like you did try to -- or did you -- did you compare Kentucky's GDP against the GDPs in Indiana, Texas, North Carolina, and in Iowa?
A. Are you looking at specific -- specific text in my table? No. You are looking at -- at the table on page 9, right?
Q. Yes. Of your -- again, we're going to say page 9 of your report which is paragraph number 22.
A. Yeah. Yeah. No. I didn't look in Kentucky's GDP.
Q. Right.
A. If I -- if I had it would've been reported here.
Q. Right. Right. And I just wanted to make sure I wasn't -- it wasn't something that you did outside of the table or something that wasn't in your report.
A. No. You're right though that it's an omission.
Q. Okay. Sure. But you did go ahead and pull the data from the Pugh study for Kentucky and record that in the table?
A. Right. And the reason why I

focused on the Pugh data is because I was really interested in understanding religiosity.

Q. Yes. Yes. Did you conduct a -- and I think -- and I'm asking this because I don't see it reported in the table. Did you conduct an evaluation of the religious density in the Louisville metropolitan area for purposes of your report?

A. I tried to, but I didn't succeeded in -- in actually getting the data properly.

Q. Okay. And was that -- did you try to use the same sources that you used for purposes of your Masterpiece study?

A. I did.

Q. Okay. And that those sources didn't provide you with satisfactory data to conduct a religious density evaluation of Louisville?

A. Yeah.

Q. Okay. Louisville has a local anti-discrimination law, correct? I mean obviously it does, but I'm just asking.

A. That's what I've heard, have you?

Q. You've heard about that? I -- we've heard about that too. And Kentucky has a

RFRA statute?

A. That's right.

Q. Yes. You recorded in your report that there was one type of jurisdiction that seemingly did not experience significant or any statistically significant change in discrimination toward same-sex weddings post-Masterpiece. And that was jurisdictions that had both a RFRA and an anti-discrimination law.

Do you recall that?

A. That's right. Yes.

Q. And I think it's been noted between 78 and 76 percent willingness to serve -- or right around there. And I assume that that really comes down to the cities of Houston and Dallas, in Texas. Is that -- were those the two jurisdictions where you -- you saw that effect?

A. No. So the list is longer. Let me find the footnote. So footnotes 13 and 14 in the JLS piece give you all of the counties in both Dallas and Indiana.

Q. And I was -- I was curious if those were -- if you included those additional cities, you -- you anticipated my next question. If you

included those additional cities in that analysis of -- of the immunity of these types of jurisdictions, to impacts by Masterpiece.

A. Sure. So if I understand your question correctly, if not, just, you know, correct me. So I want to make sure that you -- you're not somehow confused by the table. The table shows only Houston, Dallas, simply for -- simply for exemplifying and demonstrating what this might look like at the city level. But the analysis is run on all of the counties that by the time of the experiment had these kinds of local laws. Not just Dallas, and Houston, also in Indiana.

Q. Right. And -- yes, if I -- I'm looking at jurisdictions in Indiana, you have Indianapolis, Fort Wayne, Evansville, Bloomington, Muncie, South Bend, and Terre Haute?

A. Right.

Q. And I believe in Texas you were pointing to Dallas, San Antonio, Houston, El Paso, Plano, and Fort Worth. I'm looking at footnote 1 of 2 in your Harvard step, in -- in -- in the Harvard piece?

A. Okay. I'm looking at footnotes 13 in the JLS piece, but I -- it's -- it's the same -- it's the same information.

Q. Yes, it is, same information.

Okay. And so in those particular jurisdictions, the ones that we just discussed, those cities, you did not witness or record a statistically meaningful increase in the level of discrimination against same-sex couples in response to Masterpiece.

Is that fair?

A. That's correct. That's correct.

Q. Okay. And do you attribute that to -- elsewhere in your report, you have a chart that shows religious density versus likelihood of discrimination. And you show that as the density increases, you report of a greater likelihood of discrimination. So do you attribute the immunity of these jurisdictions to the Masterpiece decision in effect to a lesser religious density, or is it something else?

A. No, I don't. I want to take you to in the Online Appendix. Let me just find the table. So essentially, as often is the case with empirical pieces the journal wants you to

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provide a very brief report, and so all of the are meaningful analyses is relegated to the appendix. So a lot is going on there. Okay, so let's go to table OA4.8 on page 28 of the Online Appendix. Are you there?

Q. I'm -- I am there.

A. Okay. So I hope it's okay to guide you through this regression table.

Q. Of course.

A. Okay. It's just to answer your question, right? So what the regression table means is that you have here those columns that have numbers one to five, these are all -- each of them stand for a different model. And then wherever you see a coefficient, then that means that that variable was included in the model. And so what you see here essentially is, you see the one table where you have two different analysis. One of the evangelical congregations density and the religious congregations density, that's all groups like no matter what religion you are.

And then you see -- so you -- you can see for each of these models what happens when you introduce evangelical congregation

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density versus not, and you see that in all models, the effect of both AD, they're up there on the top and the interaction between AD and RFRA, which is the AD and then the two dots RFRA semicolon. This is a interaction, it remains statistically significant.

So even when you have evangelical congregations density or when you have religious congregations density in the model. And even when you include the interaction between same-sex and evangelical density, which is highly statistically significant. Even in those models, you still see a statistically significant effect for the interaction of AD and RFRA, which means that these variables, this specific interaction remains statistically significant even after you control for the effect of religious density.

Was that clear? I can -- I can clarify if not.

Q. I'll -- I'll repeat what I think I heard. And here's what I understood, which is that the -- the variables of how -- the variable of having both an anti-discrimination law and a RFRA law in place is sufficiently powerful

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enough that it can even overcome the impact of religious density, even religious density associated with evangelicals?

A. Well, I wouldn't say -- I wouldn't used the word overcome because that --

Q. Negate, perhaps?

A. -- that assumes some kind of maybe causal relationship. Which as we said before, I can't assume when we're talking about those differences in legislation. But it does tell you that this -- this is -- the effect of the AD-RFRA interaction does not come from any association between legal regime and religiosity rather than these are two independent effects that there is, if you will, there is enough statistical room for both of them to be highly statistically significant and powerful. And -- and -- and that's it.

And I want to qualify a little bit because sometimes analysis relates to one of the points in the rebuttal report. It's actually -- it's actually not advised to add too many factors into -- into statistical analysis. Because some -- sometimes when you actually add too many factors, then you begin to encounter

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another problem of multicollinearity between variables. And sometimes that also just alludes an obscures interpretation of coefficients.

I can give you examples, but you didn't ask me. So I wouldn't for now. But -- but -- but there is a point, what I want to say is there is a limit to how many interactions you can test in a regression model. And you know, and that's -- that's a limit there. So I -- I cannot tell you to the full extent that I would have wanted to what's the full relationship between these variables. What I can tell you that they both have independent and strong effects.

Q. Got it. And so and this is going to be -- and part of this is -- is owing to just the fact that I live in Texas, but many of the metropolitan areas that you list, I'm going to get back to the footnote in the report.

A. Yep.

Q. So Dallas, San Antonio, Austin, El Paso, and Fort Worth, you have five of the six largest metropolitan areas in state right there --

A. Uh-huh. (Witness answers in the

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affirmative.)
Q. -- comprising well over half of the state's population.
I don't -- I've never lived in Indiana, but I'm assuming that when you add Indianapolis, Fort Wayne, Bloomington, Muncie, South Bend, and Terre Haute together, you're getting a similar density of population in Indiana as well?
A. Yes.
Q. What this tells me -- and these are the jurisdictions that have that feature of both the RFRA and an anti-discrimination law that you've described as being significant and meaningful. It looks to me like the areas that were --
A. I want to correct you, they were statistically insignificant. But that's meaningful, potentially. I mean what was meaningful, what -- what -- and what you're aiming at is that there was no statistically significant effect of Masterpiece in those areas. No, they were not significant.
Q. Thank you. Yes, you're -- you're correcting my -- my elementary statistics

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knowledge that I gained about 25 years ago. So that's very -- thank you for that.
It looks to me though like the message -- the takeaway if I'm just looking at it simplistically, you know, simplistic while you're here. The takeaway is major metropolitan areas are -- that -- that have those RFRA and anti-discrimination laws are going to be somewhat immune to the effect of Masterpiece?
A. So I tested that more directly. So I have an analysis in my regression tables where I limit the analysis only to large urban areas. So only places with more than 80,000 in the population. And I still find the Masterpiece effect.
So what you forget, in a sense, is that there are big metropolitan areas also in the other states that I'm examining. And also there are big metropolitan areas also in Texas that are not included in the -- in the AD category.
So I wouldn't -- I wouldn't say with confidence what you just say. I would say that's -- that's a nice hypothesis. But what I have currently, the evidence I currently have

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would not support that this is what drives this particular effect.
Q. Combination of anti-discrimination law plus -- plus a RFRA simultaneous --
A. And as I write in the paper, I mean this -- I treat this as a preliminary finding that's super interesting and requires more research because maybe we have some kind of answer there to these conflicts. But what -- what makes, what -- what needs to curb our potential enthusiasm is that there are -- these are very specific types of RFRAs.
So in my preliminary research when I was trying to select states for the Masterpiece study, I -- I -- I just found how diverse the RFRA world is. So the RFRA of a universe is highly diverse. Some states like Kentucky did something very similar to the federal RFRA. But then you have states like Texas and Indiana that created this highly detailed law with lots of, you know, limitations. And specifically the carve-out for local anti-discrimination laws.
And that -- that is a limitation of my study that it does not have all of those

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types of RFRAs inside because it would have been impossible to compare states in all of these other dimensions. And therefore, this particular quote/unquote immunity, that I see there, I -- I -- I don't think that I can then transfer it to states or cities that have RFRAs that do not include these carve-outs.
And by the way, I do not include a lot of other things. So there are lots of limitations and conditions to using your RFRA rights in -- in Texas and Indiana that are simply not there in Kentucky. It's much simpler to use your RFRA rights in -- in Kentucky, so it seems.
Q. And so you didn't feel comfortable for the reasons that you just described, you didn't feel comfortable applying the finding to Louisville, that jurisdictions with anti-discrimination laws and RFRA laws generally don't experience an impact from Masterpiece?
A. Right. Right, if Chelsey Nelson would have been based in Austin, the report would have looked very differently.
Q. And that's because the legal regime in Austin is, you believe it's different than

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the legal regime in Louisville?
A. Yeah.
Q. Well, I can tell you Austin is certainly different than Louisville in a lot of ways. If you've ever been to Austin, you'll know Keep Austin weird is the mantra here, so...
MS. HINKLE: I have to correct you, but that's also a mantra here.
MR. BANGERT: In Louisville?
MS. HINKLE: Yeah. There --
MR. BANGERT: Oh no.
MS. HINKLE: Louisville has a lot of, People are weird, bumper stickers. You see all the time.
MR. BANGERT: Oh my goodness.
MS. HINKLE: Austin doesn't own that phrase.
THE WITNESS: I -- I -- I should update my report now.
MR. BANGERT: I'm -- I'm -- I'm deeply troubled now. Well, and I -- at least you're not in Tennessee in that case. Tennesseans insufferably claim they founded Texas. And so we -- we take great umbrage in that, but that's it. That's a fight for a

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different day.
I'm just kind of checking off my outline here, Doctor -- Professor, because I want to make sure covering things, but I'm also trying to be respectful of your time. Okay.
Okay. Maybe a few things that I can jump over.
Professor, can I beg your indulgence? I -- I may be able to wrap this up in relatively quick fashion, but if we can take about a five-minute break, I just want to consult my outline and make sure I've -- I've covered what I needed to cover and there may be a little bit more but I -- let me see if I can --
THE WITNESS: Yeah. Yeah. Let's let's then go resume on like on 12:01 my time, which is...
MR. BANGERT: Yes, that's perfectly fine. All right, I'll see you in five minutes.
THE WITNESS: Okay.

* * *
(OFF THE RECORD.)
* * *

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THE COURT REPORTER: We're back on the record.

* * *
EXAMINATION
BY MR. BANGERT:
Q. Great. Professor, thanks for coming back. I know it's late there. We'll get you out here as fast as we can. Just a few more questions to clean up the record.
One thing that you noted in your report was that you're unable to replicate the Masterpiece study today because the effects of Masterpiece will now be drowned out by other intervening events.
Is that fair?
A. Yes, it is.
Q. And so you don't -- you don't know and you really can't measure how persistent the effect of the Masterpiece decision is or was on any particular event?
A. That's correct. But -- but I can learn from other studies in answer to these questions.
Q. Okay. But in terms of the study

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that you conducted, beyond the snapshot in time that you took in June of 2018, we don't know what those attitudes looked like in July or August of 2019 -- 2018 or thereafter?
A. Right. So we do have -- I can talk about one indication in July that's consistent, not from my study from another study. And the reason why I wasn't able to continue testing was the three of the four states, legislation was introduced with respect to the expansion of religious liberty and religious exemptions. And that obviously created a conflation where even if those legislation where provoked or encouraged by Masterpiece itself, as at least some legislators were saying they were, then we can never know whether it's the -- the legislation that influenced or continues to influence or the poor decision itself.
Q. And so based on that, you -- you can't tell me based on your study whether Masterpiece had a permanent effect on attitudes of vendors toward their willingness to serve same-sex couples?
A. No. But what I can tell you is the following: First, the previous studies of

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Supreme Court decisions did find that the effect that the Court has is prolonged, the effect that the Court has on the public. So for example, Katerina Linos and Kimberly Twist that I referred to before, their 2016 paper, they studied the effect of the Court's two rulings and the, Show Me Your Papers Act and the -- what was there then the first Affordable Care Act constitutional challenge. So that was back in 2012.

And their study was identical to my own in the sense that they tested the effect shortly before and after the decision. But their study wasn't behavior, it was about attitudes. So they were trying to understand whether the -- whether the public changes in terms of the level of support it has for those different policies that were contested in court and where affirmed by the Court.

But they also had access to a public opinion survey that collected information on public attitudes both before those core decisions and also nine months after the decision. And then they found that the effect that they discovered, which was consistent with

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my own finding in the sense of the Supreme Court had an effect in the expected direction such that the public shifted in the direction of the majority ruling. So they find that the effect persisted nine months after the decision. And in fact, it even slightly increased. If you're interested in a specific reference, that's page 238 of their study.

And then in another study that was published in the Proceedings of the National Association of Science, PNAS, one of the best journals in the world, a team of researchers studied whether the gradual legalization of same-sex marriage in the United States by both court decisions and legislative act reduced implicit and explicit bias towards same-sex couples.

So the researchers there had one million responses over 12 year period. And their results across five different operationalizations, consistently find that marriage legalization, both at the level of the court and the legislation, reduces bias. And they find specifically that anti-gay's bias reduces over time in all state. So there is a

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time trend, but it reduces at sharper rates. It states that legalized gay weddings and even after this -- this reduction, there was still considerable level of bias according to their findings. But these data do show that the effect of legal changes can be substantial and prolonged.

And then the final study that I want to talk about is a study that was published. So I was actually exposed to it after I published my own work. And that's a study that was published in JAMA Psychiatry. So that's a medical journal, a very good one by a team of researchers. And that's a utilized data from 2014 through 2016 from adults, aged 18 to 64 years, in three states, that implemented laws that permitted denial of services to same-sex couples.

So in 2015, both Utah, Michigan, and North Carolina each enacted some kind of law that is essentially a religious exemption law in different fields by the way. And they had six nearby control states which were Idaho and Nevada for Utah, Ohio, and Indiana for Michigan, and Virginia and Delaware for North Carolina.

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And what they find is -- so that's like a three differences analysis. So it's both before and after. That's the first difference. Treatment versus controlled states. That's a second difference. And comparing sexual minority adults with heterosexual adults.

And they find that adults that -- individuals that identify as sexual minority, they increase in their mental distress. So they report increased levels of mental distress in the year after their states enacted laws permitting denials of service to same-sex couples as compared with both the level of mental distress before the law was enacted and the level of mental distressing in the controlling states and the level of mental distress among heterosexual couples. So in all three levels.

And this finding can be thought of as particularly worrying because what the researchers found was that sexual minority adults had already higher baseline levels of mental distress than heterosexual adults, and they don't find the same sense of change amongst heterosexual adults. And to refer to the -- to

the level -- to the -- to the long-term documentation. So this change occurred. They find -- so they find it 7 to 18 months after the implementation of these laws. So that's a considerable length of time after legislation.

Q. Sure. And so all the studies that you just mentioned, none of them were studying the wedding market in particular, were they?

A. No. That's correct. They were -- these were studies on the effect of Supreme Court decisions or on legislation on how people behave and think.

Q. And many of them were looking at relative rates of bias amongst the respondents?

A. So the first one was looking at public opinion, the second one was looking at anti-gay bias, and the third one was looking at mental distress among sexual minorities and had hetero -- heterosexual adults.

Q. Yes.

A. And then that my -- my only debt to you is that there is another study. It -- so it's a public opinion poll that was conducted one month after Masterpiece or one month and a half after I finished collecting data.

And what it examined is changes in whether the public thinks that businesses should be allowed to refuse service to gay couples for religious reasons and it'd find an increase in the level of -- so the percentage of the public, that think that this should be legal. So it's not a whole lot of time afterwards, but it's still longer than Masterpiece itself.

Q. Yes. So -- and I appreciate that information. You didn't do anything in your report though to apply and draw comparisons between the studies you just mentioned and the findings of your report.

A. So I do refer to some of these studies in my papers that are annexed when I talk about those changes.

Q. Right. But you didn't -- there's nothing -- there's nothing about those studies that would suggest that wedding vendors providing a service to individuals requesting that service, would necessarily respond or persist in their reactions to a Supreme Court case in the same way that attitudes, bias, or feelings would nec -- would persist?

MS. HINKLE: Objection to form.

THE WITNESS: What?

MS. HINKLE: Objection to form. Professor, you can answer. I was just noting my objection for the record.

THE WITNESS: So it doesn't mean that Counsel Bangert need to refer --

MS. HINKLE: No. No. You can answer the question if you understood it.

BY MR. BANGERT:

Q. I should probably re-ask it now though I think it's probably flown the coop.

There's nothing in your report -- let me strike that question. Let me ask a better question.

Unlike the studies you just mentioned, your Masterpiece study evaluates the willingness of vendors to provide a service in response to a Supreme Court decision.

Correct?

A. Yes.

Q. None of the studies you just mention, evaluated the willingness of a vendor to provide a service in response to a Supreme Court decision?

A. No. The -- the study that comes

closest is a study that looks at public opinion regarding wedding service refusal.

Q. Right. And independent -- completely independent of one's attitude, one's bias, or one's internal belief system, you've written on something called the social -- social impact regulation. The willingness of religious persons to sublimate their beliefs and attempt to accommodate those with whom they disagree when possible and when it can be squared with their religious priors.

Is that fair?

A. I wouldn't, I think, phrase it as you do, but let's see what the question is.

Q. Sure. But you are obviously familiar with this concept. You've written on it several times.

A. And glad to read my -- my work.

Q. It was -- it was -- it was fascinating, I will say. And I say that with all honesty, it was.

It's true that regardless of whether someone holds religious beliefs that may contradict or be in conflict with certain sexual lifestyles or behaviors, what you have found is

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that those religious believers will oftentimes try to find ways to reduce conflict or to accommodate those persons when possible?

A. I did.

Q. And you also, it's true that for every decision like Masterpiece Cakeshop, there are other decisions that may cut in the opposite direction in support of gay rights or same-sex wedding rights. You mentioned one, Obergefell. Correct?

A. Uh-huh. (Witness answers in the affirmative.)

Q. You also -- you're familiar, I'm sure, with the Bostock decision that was rendered by the court, the term prior to the one they just completed?

A. Yes, I am.

Q. And you would expect that those decisions would likewise have impacts and effects on public attitudes, perceptions, and beliefs about same-sex -- individuals who are homosexual or same-sex marriage?

A. I would expect that, yes.

Q. You would expect that?

A. I mean, with Obergefell, we have

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evidence that it did. Is it that's the Tankard and Paluck study and also the Ofosu study. I mean, we have evidence about that.

We also have evidence that there is reactants, I should say. I mean, for Obergefell, there was reactants. But that's the Ofosu study. There was reactants in those states that did not themselves enact legalized gay marriage themselves. People there became more hostile to gays after Obergefell than before. So that's what -- that's what the researchers found. So as you alluded to before, changes can happen in both directions.

Q. Both directions. Right. And you don't have -- you're not able to right now as we sit here to net out the effect of multiple Supreme Court decisions on attitude towards same-sex marriage or same-sex individuals. And I'm talking about Obergefell, Bostock, Fulton, Masterpiece, to net out the net effect of all of that over time.

You're not able to do that as you sit here today?

A. No. But I just want to say that not all of them are relevant in the same way.

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So Masterpiece was decided after Obergefell. So whatever effect Obergefell had is already captured within -- within Masterpiece -- that the dual and potentially conflicting effects of Bostock and Fulton, this is less -- this is more difficult because that happened afterwards.

Q. All right. But you agree the Court issuing decisions both protective of religious freedom and protective of gay rights, gay individual civil rights, can mitigate the net social impact of this decision in this area?

A. I think it's complicated to say that. First, I think, I -- I mean, I know that there was a study conducted on Bostock, but there aren't any results available yet. It's not my own. So we might have -- we may have answers on that but --

Q. But it's at least theoretically possible then?

A. You recall better than I do I'm sure that Bostock was really not about religious freedom and actually Justice Gorsuch insisted on keeping that out and not providing any hint that his expansive reading of the word sex in the Civil Rights Act will somehow create limitations

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for the practice of religious people. So and -- and I think he follows on that in Fulton.

So I think, you know, I wouldn't make any speculations. I would say that these are things that are worth testing as science -- as a scientist, I'm saying that.

Q. Okay. The Masterpiece decision was not decided on First Amendment free speech grounds, was it?

A. Not by the majority, as you know.

Q. Right. And so given the -- of course, the concurrence is talking a little bit about it. But in terms of the holding of the case, it didn't rest on free speech grounds?

A. That's correct.

Q. Now, you understand, though, that one of Jack Phillips' arguments in that case was that the First Amendment protected his right not to be compelled to speak certain messages with which he disagreed. In this case, that would be the creation of certain cakes that would -- that communicate a message that he disagreed with.

You recall that, right?

A. Yeah, of course.

Q. Right. And you and your

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Masterpiece model, you were looking more at attitudes by vendors as -- strike that question, terrible questions.

The question is: You were looking in your Masterpiece study to evaluate the effect of the Masterpiece decision on wedding vendors. And the way that wedding vendors will be impacted by the decision was -- was mediated through and filtered through the media. So the way they understood the decision ultimately would affect their reaction to it.

Is that fair?

A. Yes. Unfortunately, I think it's true for most decisions.

Q. Yes. The social impact really is still remediated by the media.

And so in the case of Masterpiece, it was not reported as a free speech decision, clearly. It was a free exercise decision.

A. Right. And I would say that it was mostly reported simply as a victory for the baker.

Q. Yes.

A. But it -- but -- but it was noted, the religious grounds, certainly.

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Q. So that -- I know. And you would expect that the way in which the wedding vendor community responded to that decision would be directly related to the way in which they understood the decision to have been framed or what the decision was?

A. As we talked about before, I don't have direct evidence on the mechanisms. The closest I have is to see how the effect expands in religiously-dense environment.

Q. Yes. Here's why I'm asking all of these esoteric legal questions, because you understand that Jack Phillips has testified, over and over again, as a baker, that he is willing to serve same-sex couples, he's willing to serve homosexual individuals, he's willing to sell them any product in his cake shop off the shelves. He simply won't create cakes that communicate a message with which he disagrees.

You understand that's his position?

A. Yes. Although I do note -- I -- I should note that I know that this was factually contested in the case and I do not consider myself able to settle with that. But I -- I did hear him say that in interviews.

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Q. Yes. And -- and I know Jack personally, and I'll tell you that is absolutely his position; that he will say that to this day. In fact, he was -- he was just to in trial back in March and that's exactly what he testified at trial. I -- in fact, one of the witnesses at trial was a homosexual individual who was a close friend of Jack's who --

A. I read -- I read your piece -- I read your piece at the National Review.

Q. Very good. So -- well, you've been reading my stuff too, so not quite as interesting as yours.

A. They're interesting.

Q. But I'll tell you he -- he certainly is willing to serve anyone, he just won't say -- to communicate a message.

And here's why I think that's interesting because I want to go back to your Online Appendix. And we were talking a little bit about changes to the -- the stimuli, changes to the -- the text of the e-mails being sent both before and after.

A. Uh-huh. (Witness answers in the affirmative.)

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Q. And in the first wave of e-mails being sent to the bakers in particularly, you note something. You say -- I'm looking at page 1 of the Online Appendix: My name is John. And my partner, Adam, and I are engaged to be married in January. We are specifically looking for a traditional tiered cake with a personalized topper. And from what we saw on your website, we think you may be just the perfect one to help us realize that dream.

And most of us would assume a traditional tiered cake with personalized topper is going to be a custom wedding cake or personalized really gives that away, right?

A. Uh-huh. (Witness answers in the affirmative.)

Q. Okay. The next paragraph, Getting down to business, what is the price range we should expect? And are there any additional services you supply that may be of interest to us? In addition, what kind of shelf product alternatives do you bake.

So I noticed that you added that in to the bakers query, and I'm assuming that you added that in because of Jack's willingness to

provide off-the-shelf goods.
Is that fair?
A. That's correct.
Q. Okay. And so --
A. That's also why I quoted the negative with referral to begin with.
Q. Yes.
A. So I was looking to see originally, I thought and that -- and I think I write this also in the paper. That's also why I have both photographers and bakers. So I was looking to see whether those nuanced differences that could be looked at and -- and would provide thought provoking ways in which vendors deal with this conflict. But that turned out to be, as I -- as you've seen, largely irrelevant.
Q. Yeah. Well, but I don't know if it was because you coded as positive, a baker who was willing to provide a service requested.
Do you know whether you coded as positive any response from the baker that they would provide shelf products?
A. Yes. And there was just -- my disappointment is that there were -- there were none.

Q. Okay.
A. So if someone ask -- if someone offered us a shelf product that -- they were coded as positive.
Q. Okay. And you also received inquiries from various bakers where you -- you coded them as, I think, positive as plus 0.5 if it was a request for additional information.
Do you recall that?
A. Yeah.
Q. Okay. And is it possible some of those bakers may have been trying to discern or decipher whether they could provide shelf products?
A. Maybe, but they were coded as positive, so I wouldn't count them as discrimination.
Q. Absolutely. So even if there was a baker who reached back out to you for more information intending to provide shelf products but having a First Amendment objection to providing a custom product, that would have been a positive baker?
A. Under my conservative coding, yes.
Q. Yes.

A. But so what is on the net --
Q. So that option --
A. So the net effect of Masterpiece that you see there is an -- that same coding policy, of course, applied to the third and the fourth wave, is it shows you that even when we're not counting those, we're seeing discrimination.
Q. As I was -- I thought it was interesting that you didn't provide that option in the third and fourth waves to the bakers.
A. So I -- I did, or at least that was -- so I asked to look at the third wave e-mail, so we know that different cakes have different costs, so can you please send us a few different options and we will go from there. So -- and we didn't ask there about like, you know, the custom topping and all of that.
So that provided a wide enough window for bakers who wanted to provide some kind of positive response to say whatever they wanted about their potential menu of cakes.
So for example, you know, you could get that and you could say, I'm happy to prepare 140 cup cakes, but I -- I don't do specialized

cakes or I can't do a specialized cake.
Q. So that's your -- that's the -- where you -- you -- at least what you testified to was that sentence: We know that different cakes have different costs, so if you could please send us a few different options and go from there, that's where you were trying to imply that shelf products would be acceptable?
A. Yes, within the -- within the necessary changes from the pre-waves to the -- to the fourth wave.
Q. Is it possible that --
A. The third and fourth wave.
Q. Is it possible that bakers receiving the third wave e-mail may have been unaware or confused by the change in text and believe that they were not actually able -- that shelf products were not one of the services being requested?
MS. HINKLE: Objection. Form and foundation.
THE WITNESS: I'm confused and that's -- I'm not sure what I'm supposed to do now.
* * *

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BY MR. BANGERT:
Q. Let me ask you this question: You agree that the third wave e-mail to same-sex couples on behalf of -- the third wave e-mail on behalf of same-sex couples to bakers does not include an explicit call to provide shelf items?
A. I think it provides a wide enough margin to do that. But again, I want to remind you that what I'm doing is I'm comparing same-sex and opposite-sex couples in the third and the fourth wave.
Q. And so your objective actually changed from the first and second wave to the third and fourth wave?
A. My objective was still to understand whether there is discrimination, but after Masterpiece, I had to insert changes in the wording of the e-mails and I kept them the same for -- across same-sex and opposite-sex couples, still allowing participants to offer me whatever they want, so creating a wide enough margin for them to be helpful if they wanted to.
But again, I mean, I'm -- I'm telling you this because this was a disappointment of mine from the businesses, that

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this doesn't really happen, not before nor after. So there wasn't -- there wasn't a change in that.
If -- if -- you know, if it would have happened before and then it wouldn't have happened afterwards, I would share your concern. But this was not close to being common. I think there may be five or so businesses that chose this option.
I thought it's going to be interesting, but it turned out to be very, very, very rare. And I share your excitement about this possibility, but the data disappointed me.
Q. So but you did make the change though and in the third wave e-mail --
A. I made it more open.
Q. You made it more open, right.
A. Yes. But that was still an option for them to propose.
Q. Right. But instead of -- would you get any -- do you recall any specific responses, negative responses, that explicitly said, I don't do same-sex weddings?
A. Very few. So there is one that you already referred to in the rebuttal report that

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was communicated over the phone. And there was none over e-mail that would either choose such explicit words as, you know, I'm --
Q. Yeah. And that's what I was trying to get at was that via the e-mail response, you didn't receive an e-mail that explicitly said, I don't do same-sex weddings?
A. No. I did get like some rude negative e-mails like, No. But that's it.
Q. Well, some vendors might just think they're efficient, right? Just a more, We're busy.
A. I mean if they were super efficient, they wouldn't answer, but they felt the need to say no.
MR. BANGERT: Very good. Okay. Professor, I have reached the end of my outline. I appreciate your time.
THE WITNESS: And I have so many other things I want to say.
MR. BANGERT: Well, I -- I -- I -- I'm sure we -- it would be wonderful to grab a cup of coffee and -- and continue the conversations but I fear that -- that you probably have work to do tomorrow.

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So Casey, I pass the witness to you.
MS. HINKLE: I don't have any questions of the witness at this time.
Thank you very much for your time today, Professor.
MR. BANGERT: All right. Very good. Well, thank you, Professor. I hope you're able to get out of your office safely.

* * *
(Witness excused.)
* * *

STATE OF KENTUCKY)
) SS.
COUNTY OF JEFFERSON)

I, JESSICA TAYLOR ROSS, a Notary Public within and for the State at Large, do hereby certify that the foregoing deposition was taken before me, via Zoom, at the time and for the purpose in the caption stated; that the witness was first duly sworn to tell the truth, the whole truth and nothing but the truth; that the deposition was reduced to digital shorthand and recorded by me in the presence of the witness; that the foregoing is a full, true and correct transcript of my digital notes and recording; that there was no request that the witness read and sign this deposition; that the appearances were as stated in the caption.

WITNESS MY SIGNATURE this 11th day of August, 2021.

My commission expires July 21, 2022.

/s/ Jessica T. Ross
JESSICA TAYLOR ROSS
Court Reporter
Notary Public, State At Large
Notary ID 602031

V/JR

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August 16, 2021

TO: Casey L. Hinkle, Esq.
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RE: UNITED STATES DISTRICT COURT, WESTERN DISTRICT OF KENTUCKY, LOUISVILLE DIVISION
Case No. 3-19-CV-00851-BJB-CHL
CHELSEY NELSON PHOTOGRAPHY, LLC and CHELSEY NELSON
VS.
LOUISVILLE/JEFFERSON COUNTY METRO GOVERNMENT, et al.
Errata Sheet for PROFESSOR NETTA BARAK-CORREN

Dear Ms. Hinkle,

Pursuant to your request for a Read and Sign, I am providing you with the transcript errata sheet for the deposition of Professor Netta Barak-Corren given in the above matter on August 4, 2021.

Please advise Professor Barak-Corren she is to read your copy transcript and complete the errata sheet (all three pages) attached hereto, have it notarized and return all three pages to me within thirty days of the date of this letter. If there are no changes to be made, please indicate there are no changes on the errata sheet, have it notarized and return all three pages to me within thirty days of the date of this letter.

Once I have received the errata sheet from you, I will then append the errata sheet to the original transcript and forward it to Mr. Bangert for safekeeping.

If you have any questions about this errata sheet procedure, please let me know.

Sincerely,

/s/ Jessica Taylor-Ross
Jessica Taylor-Ross

Enclosures (1)

UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION
Case No. 3-19-CV-00851-BJB-CHL

CHELSEY NELSON PHOTOGRAPHY, LLC and CHELSEY NELSON, PLAINTIFFS

v.

LOUISVILLE/JEFFERSON COUNTY METRO GOVERNMENT, et al., DEFENDANTS

DEPONENT: PROFESSOR NETTA BARAK-CORREN
DATE: AUGUST 4, 2021

COURT REPORTER: JESSICA TAYLOR ROSS

ERRATA SHEET FOR PROFESSOR NETTA BARAK-CORREN

TAYLOR COURT REPORTING KENTUCKY
2901 SIX MILE LANE
LOUISVILLE, KENTUCKY 40220

EXHIBIT G

The Supreme Court, the Media, and Public Opinion: Comparing Experimental and Observational Methods

Katerina Linos and Kimberly Twist

ABSTRACT

Can Supreme Court rulings change Americans' policy views? Prior experimental and observational studies come to conflicting conclusions because of methodological limitations. We argue that existing studies overlook the media's critical role in communicating Court decisions and theorize that major decisions change Americans' opinions most when the media offer one-sided coverage supportive of the Court majority. We fielded nationally representative surveys shortly before and after two major Supreme Court decisions on health care and immigration and connected our public opinion data with six major television networks' coverage of each decision. We find that Court decisions can influence national opinion and increase support for policies the Court upholds as constitutional. These effects were largest among people who received one-sided information. To address selection concerns, we combined this observational study with an experiment and find that people who first heard about the Court decisions through the media and through the experiment responded in similar ways.

1. INTRODUCTION

Can rulings from the Supreme Court, the most trusted branch of the federal government, change citizens' views so they support its decisions? Can hearing about a Court ruling prompt Americans to believe that particular

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responses among different population subgroups and why prior experimental and real-life studies come to conflicting conclusions. We argue that previous work misses a critical actor: the media. This happens even though the media, and television in particular, has been called the "most critical conduit" by which Americans learn about the Supreme Court's actions (Slotnick and Segal 1998, p. 231). In prior experimental studies, researchers assume the role of the media themselves and offer one-sided information about a major Court decision to all respondents (see Chong and Druckman 2007, pp. 638–39). Prior studies in natural settings, in contrast, often group respondents who were not exposed to any media coverage of the Court decision with those exposed to one- and two-sided coverage. We show that the intersection of the Court and the media matters critically for opinion change. The Supreme Court depends more heavily on the media to convey and translate its messages than do other elite actors. Elites in the executive and legislative branches often speak directly to the public; they also buy advertisements and hold frequent press conferences to ensure that their messages are spread widely. In contrast, the nine justices communicate their views through lengthy and complex judicial opinions. As a result, the media's role is distinctly important to the Court's influence on public opinion (Davis 1994, p. 16).

We hypothesize that the public tends to follow the Court's cues when two things occur: the Court rules on a politically salient issue and the national media present their audiences with one-sided coverage supportive of the Court decision. The media are especially likely to cover politically salient cases, as prior studies (for example, Slotnick and Segal 1998) and our data indicate. We expect some of this coverage to be one-sided and positive. We define one-sided positive coverage as messaging supportive of the Court majority; in contrast, two-sided coverage contains both supportive and critical information. Given the influence of elites' cues on public opinion (for example, Kuklinski and Hurley 1994), exposure to one-sided positive coverage of a trusted actor's views should cause some Americans to change their views, as prior experimental studies of framing effects show (for example, Nelson, Clawson, and Oxley 1997; Chong and Druckman 2007).

Three research design innovations allow us to test our theory and to add to the literature on how interactions between elites and the media shape public opinion more generally (for example, Iyengar et al. 1984; Page, Shapiro, and Dempsey 1987; Zaller 1992). First, we survey a nationally representative sample of Americans shortly before and shortly af-

ter two major and surprising Court decisions on health care and immigration, asking people for their views on the individual-mandate provision of the Affordable Care Act (Pub L. No. 111-148, 124 Stat. 119) and the "show your papers" provision of Arizona's immigration law. Prior studies of Court decisions in real-life settings are based on survey questions fielded for other purposes, such as the General Social Survey, and thus share a major limitation: there is a very long time gap, often 1 year or more, between the before sample and the after sample, and events other than the Court decision could influence opinion in this interval (Hoekstra 1995, pp. 112). We are able to show short-term effects because of the proximity of our surveys to the Court rulings, and we document long-run effects by pairing our data with other surveys.

Second, we combine public opinion data with detailed media coverage data and connect individuals' opinion shifts with the content of television programs they watch. Scholars note that "most published work on media effects does not include measures of media content" (Barabas and Jerit 2009, p. 74), and "most researchers fail to ascertain, let alone content-analyze, the media information that, they assume, their subjects encountered" (Graber 2004, p. 516). Kinder (2007, p. 158) finds it "unnerving . . . that we are still waiting for compelling demonstrations of framing effects in natural settings." Because the Supreme Court decisions we study received both widespread coverage and coverage that varied dramatically across television programs, we are able to document framing effects in a natural setting and show how Court decisions and media coverage interact to produce national opinion shifts.

Third, we combine our observational study with an experiment to better address selection effects, the key limitation of research in real-life settings. We find that Court decisions influence people who hear about them only through the media they normally use and increase overall support for policies the Court upholds as constitutional. These effects were largest among people who received one-sided information emphasizing the frame chosen by the Court majority. Two-sided coverage, in which a news program emphasizes both the Court majority's frame and alternative framings of the issue, did not confuse people about what the Court had held but did reduce the impact of the Court decision on opinion change. We also find that people who first heard about Court decisions through the media sources they normally use and people who first heard about Court decisions in the course of our experiment responded in similar ways. These findings contribute both to the literature on courts and

to the literature on public opinion formation, as effects of elites and the media have mostly been studied separately until now.

Although the two cases in our study generated appreciable media attention, they share features that make them hard tests of our theory. In both cases, the Court offered only a weak endorsement of the provisions on which we focus, the show-your-papers provision and the individual health care mandate. Both cases had been extensively debated in the media in the months prior to the rulings. Moreover, the debate tended to follow partisan lines, with Democrats supporting the health care mandate and Republicans supporting the show-your-papers provision. Thus, many respondents likely had firm opinions on immigration and health care before the Court rulings. These features bolster the generalizability of our results, as we expect stronger Court endorsements of novel issues to generate larger public opinion shifts.

2. HOW SUPREME COURT AND MEDIA CHOICES SHAPE NATIONAL OPINION

We argue that the interactions between two actors—Supreme Court justices and the national media—explain when Court decisions lead national public opinion, persuading ordinary Americans to increase (decrease) their support for policies the Court declares constitutional (unconstitutional). Studies have demonstrated the ability of high-credibility elites to lead public opinion through a process of heuristics and cue taking (see, for example, the summary in Bartels and Mutz [2009, p. 251]). Because the Supreme Court has traditionally been the most trusted branch of the federal government (Caldeira and Gibson 1992), we expect that Court rulings have the potential to provide highly influential cues to the American public. For cue taking to occur, three conditions must hold. First, the Supreme Court must decide to review a politically salient issue. Second, journalists must give extensive and one-sided coverage to the decision.[1] Third, many individuals must hear about and understand this news coverage, and some of these must change their views in accordance with the Court ruling.

1. One-sided positive coverage should move opinion in the direction of the Court ruling, while one-sided negative coverage should have the opposite effect.

2.1. Rulings on Politically Salient Issues and News Coverage

Supreme Court justices can sharply reduce the probability that they will shape public opinion by refusing to take on politically salient issues. The Court has significant discretion over the cases it reviews and grants fewer than 1 percent of the petitions it receives.[2] Prior work establishes that when the Court uses its discretion to deny review, limited news coverage typically follows, even on politically salient and controversial issues (Slotnick and Segal 1998). In turn, limited national coverage leaves national opinion unchanged (Hoekstra 2003).

Conversely, many cases taken on by the Supreme Court receive extensive coverage at the time of the Court ruling. Cases on politically salient topics, especially those involving individual rights, tend to receive disproportionately more media coverage relative to their share of the Court docket (for example, Solimine [1980]; Bowles and Bromley [1992]; see Persily, Metzger, and Morrison [2013] for a discussion of the importance of the Affordable Care Act ruling). In addition, cases that attract many amicus briefs and cases involving multiple dissents garner more coverage, as journalists often consider these important and controversial, and thus newsworthy (Sill, Metzgar, and Rouse 2013, p. 74). However, prior studies cannot tell us why some widely covered cases change Americans' views, while others do not, so we turn to this point next.

2.2. One-Sided Frames and Americans' Views

While extensive news coverage is necessary for Americans to become aware of a Court ruling, we argue that the nature of this coverage determines whether they will take cues from the ruling and update their opinions. More specifically, the arguments that justices develop to support their conclusions and journalists' choices about how to present, add to, or challenge the Court's argumentation are critical. To explain how lengthy and complex legal opinions are translated into short sound bites, we combine general theories about journalism with work specific to journalists covering the Supreme Court.

The Court is distinctive among elite actors in the United States, in that the public hears about its rulings only indirectly. After the Court issues its complex and lengthy opinions, the media decide how to translate those opinions to the public. In contrast, videos of members of Congress or

2. Supreme Court of the United States, Frequently Asked Questions (http://www.supremecourt.gov/faq.aspx).

the president speaking are often aired on the news—there is less need for the media to translate what they say, because the public can hear these messages directly. This elevates the media's role to that of a critical mediator in studies of Court influence on public opinion (Davis 1994, p. 16; Franklin and Kosaki 1995). We focus on television coverage, as television remains the main source of news for most Americans, even in the digital era (Pew Research Center 2011), and, as we note above, is viewed as the "most critical conduit" for information about the Supreme Court (Slotnick and Segal 1998, p. 231). As reporters covering the Court seldom have the time to tell as many stories as they would like (Slotnick and Segal 1998, p. 47), media outlets will pick one or only a few frames likely to resonate with viewers.

Although there are three possible options for coverage—one-sided positive, two-sided, or one-sided negative—we expect that the media will typically choose either one-sided positive or two-sided coverage. Scholarship on journalists covering the Court suggests that they may understand their role as explaining, rather than criticizing, Supreme Court opinions (Slotnick and Segal 1998, p. 21; Davis 1994, p. 20). By reporting on the Court's actions through the frames chosen by the Court majority, journalists may end up presenting one-sided, largely uncritical coverage. Early studies suggest that television coverage mostly presents the Court majority's position and does not differ significantly among the networks (Katsh 1983; Davis 1987; Slotnick and Segal 1998).

A different theory of journalism, indexing theory, in turn suggests that journalists seek to avoid acting as mouthpieces for the administration and instead turn to diverse government elites and interest groups to construct frames: they index their reporting to the "magnitude and content of conflicts among key government decision-makers [and other powerful players]" (Bennett 1996, pp. 376–77). Because politicians and interest groups are eager to voice both supportive and critical viewpoints on controversial Court decisions, indexing theory leads us to expect two-sided coverage. We would thus expect to see either one-sided positive coverage or two-sided coverage rather than one-sided negative coverage. That said, it is possible that journalists working for partisan cable networks, namely, Fox News and MSNBC, consistently emphasize frames supportive of their networks' ideologies (Meader 2013) that could be entirely hostile to the Court, so it is important to study current coverage data. Such variations in media coverage could shape the persuasive effects of Court decisions, as we explain next.

2.3. Extensive One-Sided Media Coverage and Persuasion

Journalists' choices about the frames they select, and the considerations they emphasize, should influence public responses to Court decisions. Many survey experiments indicate that individuals who receive one-sided frames change their opinions accordingly, while individuals who receive two-sided, competing frames are more likely to retain their original views (for example, Druckman 2001; Chong and Druckman 2007, 2013). However, these experimental studies involve fictitious information transmitted to respondents by researchers; as we note above, Kinder (2007, p. 158) says that "we are still waiting for compelling demonstrations of framing effects in natural settings." If Supreme Court decisions receive both extensive coverage and a variety of one-sided and two-sided coverage, we should be able to identify framing effects following actual Supreme Court cases. By separating respondents who receive no information, one-sided information, or two-sided information from the media they regularly use, we should be able to reconcile experimental findings with real-life results.

Hypothesis 1. People who receive one-sided information supportive of the Court majority's ruling should respond more positively than people who receive two-sided information (that also includes critical frames), both in experiments and in natural settings.

3. RESEARCH DESIGN

Prior studies of the Supreme Court's influence in natural settings rely on data generated for other purposes, and long lags often separate the before and after waves. In contrast, the survey research firm YouGov fielded our two-wave, nationally representative survey shortly before and shortly after two major Court decisions. Survey respondents received the first wave of questions in May 2012, about 5 weeks prior to the rulings, and the second wave in the days after the rulings. For each survey, 1,300 respondents completed wave 1; 87.5 percent of these individuals completed wave 2 of the health care survey, and 82.3 percent completed wave 2 of the immigration survey. YouGov reweighted these survey responses to create a nationally representative sample and provided us with complete responses for 2,000 subjects (1,000 per study).

We combined these public opinion data with news coverage data. Scholars note that most published work on media effects does not, in

fact, record or analyze media coverage (Barabas and Jerit 2009, p. 74; Graber 2004, p. 516). The few prior on-point studies find that few Americans respond to events that receive limited national coverage (for example, Barabas and Jerit 2009; Jerit, Barabas, and Bolsen 2006; Nicholson 2003; Price and Czilli 1996), and only the opinions of highly attentive subgroups are likely to change in response to these events (Barabas and Jerit 2010).

By focusing on Supreme Court cases that received moderate to high levels of coverage, we can study how a broader cross section of Americans respond and examine whether factors besides the volume of coverage—such as the frames used by different news sources—influence opinion change. We analyze how six different television networks covered two major Supreme Court cases and connect individuals' opinion shifts to frames to which they were (or were not) exposed, to provide the first comprehensive study of framing effects in a natural setting. Section 4 outlines how we content analyzed news media coverage.

While studies in natural settings have important external-validity advantages and provide "more realistic news scenarios" than what experiments typically offer (Chong and Druckman 2011, p. 254), important concerns about selection arise. The best observational studies, for example, compare people who hear about an event with people who do not. But it is possible that avid news followers care deeply about political issues and current events and thus respond especially strongly to new information about political events. Similarly, we worry that that people who choose to watch Fox News tend to be conservative, or those who watch MSNBC tend to be liberal, and may be more likely than others to respond favorably to ideological messages.

To address selection concerns, we include careful controls, study changes in opinion (to account for individuals' views prior to the Court decision), and combine our observational study with an experiment. In this experiment, we randomly assigned people to receive different information about each case to see whether people who receive information through the media they choose and people randomly assigned to receive similar information respond in similar ways. For each study, 40 percent of respondents were randomly assigned to the no-reminder group (n = 400) and did not receive any information about the content of the Court decision from us. The remaining respondents were divided into three equally sized groups and randomly assigned to receive different reminders about the decision (n = 200 per group). Our reminders drew on the

major arguments developed by the justices. However, we presented our respondents with short summaries of key points, rather than extensive legal opinions, in an effort to make the information offered through the experiment comparable to the information a respondent might receive through the media she typically uses.

In sum, we expect that people who receive one-sided information that the Supreme Court has judged a law to be constitutional should increase their support for the law, whether they receive the information from the media they regularly use or from a researcher. In contrast, people who receive two-sided information should respond less positively; indeed, the net effect might be zero or even negative, depending on the relative strength of the competing frames. We also expect that when information is repeated in the course of an experiment to people who have already received it through the media they regularly use, we should not see a further response (Druckman and Leeper 2012, p. 889). Table 1 outlines these predictions.

We expect two-sided frames to have smaller effects than one-sided frames; these attenuated effects could be zero if the two frames are equally strong or even negative if the counterframe is stronger than the main frame. Details about our question wording and study design and our robustness checks are in the online appendix.

4. MEDIA COVERAGE OF THE HEALTH CARE AND IMMIGRATION DECISIONS

Our goal is to examine whether variation in the frames that television programs use influence Americans' responses to Court decisions. In this section, we describe how the news media covered the health care and immigration decisions and provide evidence that media coverage of these decisions varied, with some news programs offering one-sided positive coverage and others offering two-sided coverage.

That said, there are some prerequisites to national opinion change: we expect to see national opinion shifts only when an event receives at least moderately high levels of and moderately clear news coverage. The immigration ruling received a moderately high level of news coverage, while the health care ruling received a high level of news coverage. In fact, media coverage of both health care and immigration issues spiked after the Supreme Court decisions to levels not seen before or since; no other actor or event, including the presidential debates, focused as much media attention on these issues as the Supreme Court. While both rulings

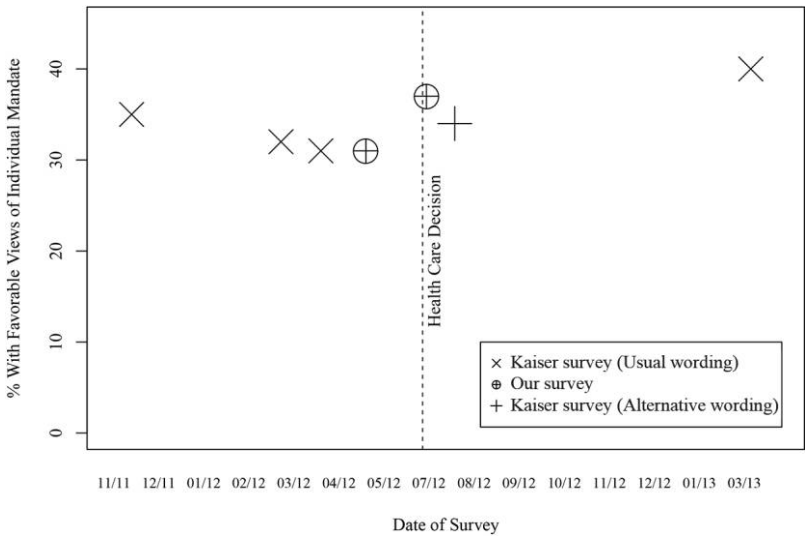

Figure 3. Support for the individual mandate over time

from 29 percent before the decision, in mid-May 2012, to 35 percent in the days after the decision, in late June 2012 ($p < .05$). We tried to field our surveys as close to the Court decision as possible so that our before-and-after comparison reflects responses to the Court decision rather than responses to other intervening events.

We complement our data with data collected by the Kaiser Family Foundation (2013) to illustrate that our short-term effects persisted for many months after the decision. Although the Kaiser Family Foundation used a different sampling technique and phrased its questions differently than we did, it is reassuring to see that its findings are very similar to ours. More specifically, the Kaiser surveys suggest that support for the individual mandate increased from 32 percent in March 2012 to 40 percent in March 2013 and thus that the short-term bump we report persisted for many months after the Court decision. In addition, the Kaiser survey suggests that there was no upward trend in support for the mandate in the months preceding the Court decision, which helps reassure us that the decision constituted a turning point.[7]

We see a bump in support for the show-your-papers provision in our data following the immigration decision, which upheld this provision of

7. Kaiser used distinctive question wording in the survey immediately after the ruling (July 2012), marked with a cross in our graph.

Arizona's immigration law, but it is small and not statistically significant. The discrepancy may well be because coverage of the immigration decision was more limited and confusing than coverage of the health care decision. As a result, only a minority of Americans understood that the Court had upheld the controversial show-your-papers provision, while others did not know, and still others believed that the provision had been struck down.

It is, therefore, important to not only measure aggregate opinion change but distinguish people who heard and understood the decisions from those who did not. Current best practice suggests that questions about exposure to news coverage, rather than questions about knowledge of the content of the news coverage, should be used to distinguish treated from untreated subjects (Druckman and Leeper 2012). Had we used questions measuring knowledge—such as those in our survey asking people whether the provision had been upheld or struck down—as a measure of treatment, we may have introduced bias, because respondents can use their prior beliefs to make sense of news coverage. Severe bias is particularly likely when news coverage is confusing, as in the immigration study. For example, people who strongly supported the show-your-papers provision in wave 1 were far more likely to (correctly) indicate that it had been upheld, relative to people who opposed the provision in wave 1.

While prior studies highlight that exposure, rather than knowledge, is the correct way to distinguish treated from untreated respondents, there is limited research on which measures of exposure are best. Indeed, some of the best prior work is concerned with only whether someone was exposed to a particular media source, rather than to news of a specific event, and thus a simple binary measure of exposure is appropriate (for example, DellaVigna and Kaplan 2007; Gerber, Karlan, and Bergan 2009).

Because we are interested in exposure to specific events, and in general exposure to a news source, we had to construct new measures of exposure. As this part of the coding is novel and somewhat subjective, we present four exposure cutoffs so as to be as transparent as possible about how our coding of exposure influences our results. The health care decision received widespread and straightforward coverage, while the immigration decision received moderate and somewhat confusing coverage.[8]

8. For theoretical reasons, we believe that it is appropriate to use a high threshold of exposure for the health care decision and a moderate threshold of exposure for the immigration decision. We show all thresholds for both decisions in the interest of transparency.

EXHIBIT H

Research Article

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The Effect of a Supreme Court Decision Regarding Gay Marriage on Social Norms and Personal Attitudes

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Abstract

We propose that institutions such as the U.S. Supreme Court can lead individuals to update their perceptions of social norms, in contrast to the mixed evidence on whether institutions shape individuals' personal opinions. We studied reactions to the June 2015 U.S. Supreme Court ruling in favor of same-sex marriage. In a controlled experimental setting, we found that a favorable ruling, when presented as likely, shifted perceived norms and personal attitudes toward increased support for gay marriage and gay people. Next, a five-wave longitudinal time-series study using a sample of 1,063 people found an increase in perceived social norms supporting gay marriage after the ruling but no change in personal attitudes. This pattern was replicated in a separate between-subjects data set. These findings provide the first experimental evidence that an institutional decision can change perceptions of social norms, which have been shown to guide behavior, even when individual opinions are unchanged.

Keywords

social influences, social perception, prejudice, attitudes, intergroup dynamics, open data, open materials, preregistered

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After the Supreme Court legalized interracial marriage in the 1967 decision *Loving v. Virginia*, Americans' support for interracial marriage climbed from under 25% to its present-day levels of over 80% (Newport, 2013). Analysts suggested that the Court's decision helped fuel the shift in public opinion (Marshall, 1987; Schacter, 2009). Whether and when institutional decisions actually change individual attitudes are questions that social scientists have long pursued. Studies have suggested that under some conditions, institutions are responsible for changing personal attitudes about social or political issues (e.g., Bartels & Mutz, 2009; Beaman, Chattopadhyay, Duflo, Pande, & Topalova, 2012; Bishin, Hayes, Incantalupo, & Smith, 2016; Clawson, Kegler, & Waltenburg, 2001; Hoekstra, 1995; Mondak, 1992). However, attitudes do not always change easily, particularly when they concern contentious issues. Personal experiences and religious and political views often anchor individuals' attitudes, which makes them less flexible (e.g., Johnson & Eagly, 1989; Jost, Federico, & Napier, 2009; Prentice, Gerrig, & Bailis, 1997; Sherif & Hovland, 1961).

Questions about the social influence of institutional decisions are nearly always posed one way—does the decision change an individual's personal attitude toward the issue? We hypothesized that institutional decisions may change a different, also consequential viewpoint: an individual's perception of social norms. Norm perceptions are impressions of what opinions or behaviors are common among, or considered desirable by, a group of people (Cialdini & Goldstein, 2004). The perception that "most Americans support interracial marriage" is an example of a perceived social norm (contrasted with "I

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support interracial marriage," which is a statement of an individual attitude). Extensive research has demonstrated that norm perceptions are consequential because they can guide important individual behaviors, such as energy conservation, voting, and alcohol consumption (Tankard & Paluck, 2016). Individuals use norms as a guide to behavior because they are motivated to be accurate in their social judgment and also because they wish to avoid social rejection (Cialdini & Goldstein, 2004). Thus, social norms may not always align with personal attitudes—individuals may perceive shifts in collective opinion without changing their own mind (Paluck, 2009; Sherif, 1936). In some cases, perceptions of norms motivate changes in attitudes (Stangor, Sechrist, & Jost, 2001). But despite their relevance for understanding social and behavioral change, perceived norms are rarely measured in public opinion polls.

We predicted that institutional decisions could alter perceptions of social norms because these perceptions are known to be dynamically updated over time as individuals take cues from their environment, such as the public behavior of group members or summary information about a group (Miller & Prentice, 1996; Paluck & Shepherd, 2012). We theorized that institutions (defined as entities that "govern, educate, or organize a reference group and their social interactions"; Tankard & Paluck, 2016, p. 192) are an additional cue regarding the content and direction of social norms.

First, because they represent collectives, institutions may be able to change how individuals perceive the group—for example, perceptions of what American people believe now (Tankard & Paluck, 2016). These are perceived norms of the status quo. Although an institution such as the Supreme Court is intended to be insulated from public opinion pressures, individuals may believe that it considers public opinion purposefully, to maintain support, or incidentally, because individual judges are subject to the same social forces as the public (e.g., Friedman, 2009; Mishler & Sheehan, 1996; Rosenberg, 1991). For either reason, if individuals believe that an institution has access to accurate information about public opinion, or if an institution is democratic in nature, they may view an institutional decision as a signal of where the public stands and update their own subjective perception of public opinion.

Likewise, institutional decisions may be understood as an expert projection of the direction in which public opinion or behavior will move (or should move) in the future. At least for institutions that are trusted to determine appropriate behavior and represent a group's best interests (i.e., "legitimate" institutions; Tyler & Jackson, 2014), we hypothesized that decisions may signal where collective opinion is headed. These perceptions are a type of directional perceived norm, that is, what the collective will be thinking or doing in the future.

Additional factors undoubtedly contribute to institutional influence on perceived social norms. For example, when an institution is highly visible, individuals are aware that many other group members are simultaneously observing its decisions. This awareness of shared attention, reinforced by mass media, may contribute to changes in perceptions of what the group currently believes or will believe (Chwe, 2003; Shteynberg, Bramlett, Fles, & Cameron, 2016). Our goal at present was not to disentangle various explanations for the influence of institutional decisions on social norms, but rather to provide the first experimental test of whether a causal relationship exists between major institutional decisions and perceived social norms.

As we have noted, personal attitudes also respond to institutional decisions under certain conditions. First, institutions seem to influence personal attitudes through the persuasiveness of the reasons behind their decisions and by engaging individuals in deeper thinking—not, by contrast, through a signaling process in which an institutional endorsement is accepted unthinkingly (Bartels & Mutz, 2009). Thus, for issues such as gay marriage on which attitudes are guided less by deliberative reasoning and more by moral instinct (Powell, Quadlin, & Pizmony-Levy, 2015), institutions are less likely to have strong effects on personal attitudes. Second, studies show that attitudes are influenced when the institutional decision is experienced personally by an individual—for example, if the decision affects local community conditions (Hoekstra, 2000). In India, for example, institutional quotas that increased citizens' exposure to local female politicians changed attitudes toward female leadership (Beaman et al., 2012; see also Flores & Barclay, 2016; Kreitzer, Hamilton, & Tolbert, 2014).

Given that personal attitudes are more strongly tied to ideology and personal experience than to social norm perceptions (e.g., Johnson & Eagly, 1989; Jost et al., 2009; Prentice et al., 1997; Sherif & Hovland, 1961), some of the conditions that limit institutional influence over attitudes may not apply to institutional influence over perceived norms. Thus, in the studies reported here, we sought to identify whether an institutional decision could influence social norm perceptions, even under conditions in which one might not expect attitude change.

Understanding the causal effects of an actual institutional decision is difficult without the ability to randomize exposure to the institutional decision (see Beaman et al., 2012) or without prospective time-series data testing how individuals change prior to and following an institutional decision (Bishin et al., 2016). The present research capitalized on the respective strengths of experimental and time-series strategies, presenting evidence for the causal influence of the U.S. Supreme Court's 2015 *Obergefell v. Hodges* ruling on

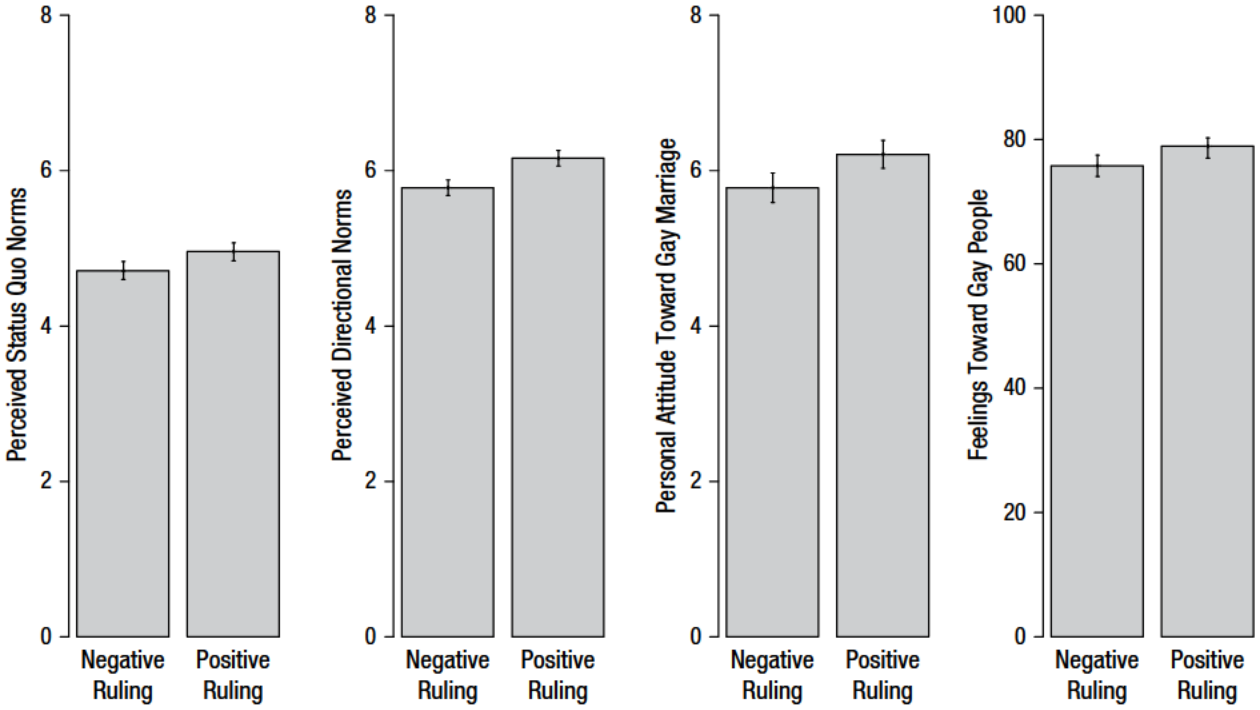

Fig. 1. Results from Study 1a: mean ratings of perceptions of status quo norms, perceptions of the direction and speed of social change in support of gay marriage, personal attitudes toward gay marriage, and feelings toward gay people. Ratings are shown as a function of Supreme Court–ruling condition. The first three measures were rescaled for ease of comparison. Error bars show 95% confidence intervals.

Material). We found evidence of a stronger effect of the anticipated Supreme Court decision among participants who reported more trust in the Court and belief in its representativeness of public opinion in the United States (Table S13). A limitation is that perceptions of the Supreme Court were measured at the end of the study and were affected by the manipulation (see Table S4 in the Supplemental Material). The study manipulated perceptions of the Supreme Court decision without the accompanying real-world influences of media and peer reactions. Participants in both conditions imagined a variety of reactions, and imagined media reactions did not account for the effect of the manipulation on norms or attitudes (see Sections F and H in the Supplemental Material). Thus, our small but statistically significant effects of a positive Supreme Court ruling for legalized gay marriage on social norms perception support the hypothesis that perceived social norms can change in response to the likelihood of the institutional decision itself.

The fact that participants imagined negative and mixed reactions to both positive and negative rulings also suggests that they were not trying to please the experimenters, although this possibility cannot be ruled out. However well-controlled, these types of contrived survey experiments may induce participants to personally engage with and understand an institutional decision to an extent that they otherwise might not (Egan & Citrin, 2011; Unger, 2008) and can serve only as an approximation of a real-world event. For this reason, we conducted a prospective longitudinal study of the actual Supreme Court decision on gay marriage, to capture any changes in attitudes and perceived norms.

Study 1b

Method

In a five-wave time-series study that followed the same individuals prior to and following the June 2015 U.S. Supreme Court ruling on same-sex marriage, we tested whether the ruling corresponded with a positive shift in perceived social norms and attitudes in support of gay marriage and of gay people. We also collected a separate between-subjects replication data set.

Participants. Participants (43.3% female, 56.7% male; mean age = 33.93 years) were recruited online via Amazon Mechanical Turk for the within-subjects longitudinal component of the study. The final sample consisted of

Table 1. Dates of Longitudinal Survey Waves Surrounding the Announcement of the Supreme Court Ruling on Gay Marriage (Study 1b)

Wave	Launch date	End date
Before announcement of Supreme Court ruling		
1	March 10, 2015	May 28, 2015
2	June 10, 2015	June 14, 2015
3	June 22, 2015	June 24, 2015
After announcement of Supreme Court ruling		
4	June 27, 2015	July 3, 2015
5	July 18, 2015	July 23, 2015

Note: The Supreme Court ruling was announced at 10:00 a m. on June 26, 2015.

1,063 participants after the exclusion of 269 participants who failed attention checks. As in Study 1a, participants were retained in the sample if they were U.S. citizens, did not indicate an LGBTQIA identity, and passed a series of attention checks. Including participants who did not pass attention checks does not change any of the study's results. The recruited sample size was determined by the power analysis in Study 1a and through additional consideration of the repeated measures design and of attrition rates in a study with similar follow-up methods also conducted on Mechanical Turk (Christenson & Glick, 2013). On average, these participants reported consuming mass media news about 3 to 4 times per week, with a 57% majority reporting daily consumption, which suggests that few participants were unaware of the ruling. For the between-subjects time-series data collection, an additional 545 adult participants (45.5% female, 54.5% male; mean age = 32.36 years) were recruited in total across Waves 2 through 5 of data collection (an additional 131 participants were excluded for failing attention checks). This resulted in a combined sample of 1,608 participants in the between-subjects data set.

Within-subjects time-series procedure. Participants for the within-subjects portion of the study were recruited to complete an initial survey (Wave 1) on Amazon Mechanical Turk via an invitation that was posted on the Web site each Wednesday from March 10 to May 28, 2015. The timing of this initial recruitment phase and all subsequent measurement waves was based on the expectation that the Supreme Court ruling would be issued in late June. All participants who were retained in the Wave 1 survey were subsequently invited by e-mail to participate in the two other surveys prior to the ruling (Waves 2 and 3) and the two surveys following the June 26 ruling (Waves 4 and 5; see Table 1 for dates). To maximize participant retention in the two postruling data collections, we sent two identical invitation e-mails on separate days for Waves 4 and 5; otherwise, each invitation to participate in a follow-up wave was identical (a recruitment strategy adapted from Christenson & Glick, 2013; see Section H in the Supplemental Material). Participants were compensated 30¢ at Wave 1 and 50¢ at each follow-up wave, with a $1 bonus for completing all waves.

Our rate of retention across Waves 2 through 5 was on average 54.4% of all respondents, a rate comparable with that of other longitudinal time-series studies conducted on Amazon Mechanical Turk (Christenson & Glick, 2013). In Waves 2 through 5, responses were retained if the participant passed that wave's respective single attention-check question. In Wave 5, about 12.3% of the sample guessed the general purpose of the study as it related to the Supreme Court ruling. Their responses are included in all our reported findings; excluding their responses does not affect our results.

Between-subjects time-series procedure. After the conclusion of Wave 1 of the longitudinal component on May 28, 2015, we issued an invitation each Wednesday for a new set of participants to complete the survey, recruiting new respondents weekly through July 22, 2015. The survey was identical to the Wave 1 survey given to the within-subjects sample. This additional sample, combined with respondents from Wave 1 of the within-subjects portion of the study, formed a time-series data set in which new participants were surveyed each week from March 10 through July 22, 2015. We collected this cross-sectional data set to eliminate suspicions about the research hypotheses within one set of respondents and to test whether the effects would be replicated in a repeated panel sample.

Survey items. Participants responded to the same dependent measures as in Study 1a. In the within-subjects data set, the behavioral measure was not repeated in follow-up waves.

Results

Across several specifications that accounted for survey attrition in different ways, we found robust evidence that the Supreme Court ruling was associated with a significant shift in perceived (present and future) social norms in support of gay marriage. We did not find evidence of change in personal attitudes toward gay marriage or in ratings of gay people. This pattern emerged when comparing the survey waves measured immediately before and after the ruling, when implementing a regression-discontinuity design to account for trends over time, when incorporating inverse-probability weighting to address attrition, and when analyzing the separate cross-sectional sample.

First, using paired-samples *t* tests to compare the responses of all participants who completed both the survey wave conducted several days prior to the ruling

EXHIBIT I

1-10-2018

Backlash or a Positive Response? Public Opinion of LGB Issues after Obergefell v. Hodges.

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RUNNING HEAD: Public Opinion after *Obergefell v. Hodges*

Backlash or a Positive Response? Public Opinion of LGB Issues after *Obergefell v. Hodges*

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Our findings, however, are only generalizable to the Nebraska context. The demographic, makeup of Nebraska differs from other states in important ways, such as racial and ethnic diversity; a more racially and ethnically diverse sample could result in more positive attitudes on LGB rights related to discrimination yet more negative attitudes related to same-sex marriage (Lewis, 2003; Moore 2010). Another consideration is that our data also come from two waves of a longitudinal survey that had two separate, independent samples. A panel study with the same respondents reporting their opinions of LGB issues before and after the ruling would provide a more complete examination of how the ruling affected individuals' opinions. Also, the 2015 wave was conducted very shortly following the *Obergefell* decision (the first mailing was sent on August 12th and the decision was June 26th). Given that people's attitudes about social and legal changes may not shift simulteanously with the changes themselves, it could be that we did not adequately capture negative sentiments just two months after the decision. Finally, it is important to note that the survey did not ask questions about transgender rights. This is an important limitation to highlight particulary given the degree to which scholars contend that hostility over transgender rights and the introduction of "bathroom bills" reflects a backlash to increasing gains of LGBT people, especially marriage equality. Indeed, research indicates less support for rights for transgender people compared to gays and lesbians (Lewis et al., 2017). Despite these limitations, our study uses a unique dataset from a more politically conservative state to understand how the Supreme Court ruling on same-sex marriage affects public opinion on this and other LGB issues.

Analysis Plan

We first report descriptive results of Nebraskans' opinions of same-sex marriage, adoption of children by gay and lesbian couples, and protections for gay men and lesbians from housing and job discrimination for 2013 and 2015. Using two-tailed t-tests, we examine if the distribution of responses to these questions significantly differ between 2013 and 2015. Next, in regression models (logistic for favor/oppose outcomes and multinomial for the nominal outcomes of the same-sex marriage question–favor/favor civil unions only/oppose) using pooled 2013 and 2015 NASIS data (n=2,751), we examine if the Supreme Court ruling affected opinions of LGB issues. The main independent variable is a dichotomous indicator of pre- (2013) or post-ruling (2015). Control variables in the models include respondent demographic, political, and religious characteristics. Lastly, in additional regression models, we examine if opinions of same-sex marriage significantly differed pre-/post-ruling for men, women, Democrats, Republicans, Independents, those who do and do not identify as born again Christian, and those who do and do not personally know an LGB person.

To answer our second research question, we use chi-square tests with the NASIS 2015 data to examine how people's perceptions of the Court's ruling reflecting marjority American and Nebraskan opinion relates to their own opinions of same-sex marriage. For our third research question, we use chi-square tests using NASIS 2015 data to examine if support for same-sex marriage differs by how often people discuss LGB issues.

Weighting

For our analyses, we use weighted NASIS data with the corresponding *svy* commands in Stata14. The data are weighted to account for within-household selection, unit nonresponse, and population characteristics. The weights for both 2013 and 2015 were for household size and sex, age, and region of Nebraska using 2010 US Census data (NASIS 2012-2013 Methodology

Report; NASIS, 2014-2015 Methodology Report). Table 2 displays the weighted demographic, political, and religious characteristics of the completed NASIS samples for 2013 and 2015. T-tests indicate no significant differences in respondent characteristics between the 2013 and 2015 data. One important exception is that more respondents in 2015 reported knowing an LGB person than in 2013. We control for this in the regression models and also return to the importance of this fact in the discussion in light of our other findings. The 2015 NASIS also asked whether respondents heard or read about the *Obergefell v. Hodges* ruling, and 92.6% of the sample reported yes,[1] meaning the vast majority of respondents were aware of the ruling.

<INSERT TABLE 2 HERE>

Findings

Table 2 shows the distribution of Nebraskans' opinions of LGB issues for 2013 and 2015. Support for all four LGB issues that we examined—same-sex marriage, adoption by gay and lesbian couples, and protections from discrimination in housing and employment—was nominally higher in 2015 than 2013 indicating there was not a backlash to the Court's decision in terms of public opinion. T-tests show that significantly more Nebraskans favored same-sex marriage in 2015 than 2013 (40.5% vs. 48.04%; $t=-3.13$, $p<0.01$) and significantly more Nebraskans favored policies to protect LGB people from housing discrimination in 2015 than 2013 (71.78% vs. 77.10%; $t=-2.55$, $p<0.05$). Differences in opinions between 2013 and 2015

1. A logistic regression model showed that people from Omaha and Lincoln and those who know an LGB person were significantly more likely to have heard about the ruling. No other demographic, political, or religious characteristics were statistically significant in the model, suggesting that other subgroups did not differ in the likelihood that they heard about the ruling.

EXHIBIT J

Same-sex marriage legalization associated with reduced implicit and explicit antigay bias

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The current research tested whether the passing of government legislation, signaling the prevailing attitudes of the local majority, was associated with changes in citizens' attitudes. Specifically, with ~1 million responses over a 12-y window, we tested whether state-by-state same-sex marriage legislation was associated with decreases in antigay implicit and explicit bias. Results across five operationalizations consistently provide support for this possibility. Both implicit and explicit bias were decreasing before same-sex marriage legalization, but decreased at a sharper rate following legalization. Moderating this effect was whether states passed legislation locally. Although states passing legislation experienced a greater decrease in bias following legislation, states that never passed legislation demonstrated increased antigay bias following federal legalization. Our work highlights how government legislation can inform individuals' attitudes, even when these attitudes may be deeply entrenched and socially and politically volatile.

prejudice | attitudes | intergroup dynamics | norms

Social norms can exert a strong influence on attitudes and behaviors (1, 2). People often modify their views and actions to align with the perceived norms in their environment (3). Norms are not necessarily explicit and often must be inferred (4). People tend to infer and reinforce social norms through social interaction (5, 6). Given the implied nature of social norms, the attitudes and behaviors deemed acceptable are prone to change over time. Even when an individual personally disagrees with a normatively accepted behavior, they may uphold it through cognitive dissonance (7). Specifically, to the extent that one consistently modifies their behavior to be congruent with perceived norms (8, 9), personal attitudes, including prejudice toward social groups (10), might change over time as well. The current research focuses on local government's role in signaling such norms. Specifically, we examine whether the local changes to government policy supportive of a marginalized social group informed the biases of citizens toward that group.

There are multiple reasons why legislation passed by a democratic government might be perceived as a norm. The literal translation of democracy is the "rule of the people," and theoretically a democracy is a system of government in which elected representatives create laws aligning with the interests of the majority of the population. Realistically, while the way in which legislation is formed is far more complex, people within a democratic system may generally perceive laws to reflect the will of the people. Consequently, they may interpret enacted legislation as consistent with the values and beliefs of the majority. Consistent with this view, people do infer that policies enacted by a group reflect the group's approval, even if the policy was not enacted by a majority opinion (11). Therefore, enacted legislation might be perceived as a strong signal of current local norms. Should any legislation impact the outcomes of specific social groups, this legislation might be perceived as reflecting prevailing societal attitudes toward those groups more broadly. Indeed, individuals update their perceptions of social norms over time based on environmental cues (12). Similarly, from local legislation, individuals can learn to what extent they may be in the majority or minority, and therefore how acceptable it is to express any attitudes regarding those social groups.

The opportunities to examine the impact of government policy on attitudes toward marginalized social groups are very rare, yet there is some evidence that government policy can change attitudes. For example, there was a 60% increase in support for interracial marriages following legalization in 1978 (13), a change various scholars have partially attributed to the Supreme Court's verdict (14, 15). In more recent history, following the 2016 United States presidential election of Donald Trump, participants reported an increase in the acceptability of prejudice toward stigmatized groups (16). Furthermore, experimental work is supportive of these conclusions, finding that consensus information causes changes in participants' attitudes (17, 18).

Same sex marriage legalization is a unique phenomenon providing an opportunity to study the relationship between local legislation and citizens' attitudes. This is because, while the US Supreme Court adjudicated that the right to marry was fundamental and inalienable (19) on June 26th of 2015, 35 states and Washington, DC, had passed state level same sex marriage legalization in some form before this date at different times over the previous 11 y. This pattern of legalization over time provides a natural, quasi experimental, multiple group, interrupted time series, with staggered treatments across groups (states), a design that mitigates many of the threats to causal conclusions typically associated with observational data (20–22).

Significance

How does the legislation passed by governments influence citizens' attitudes? We take advantage of the staggered manner in which same-sex marriage legalization occurred in the United States to examine this question with regard to antigay bias. By geolocating approximately 1 million respondents as they completed measures of bias over a 12-y window, we tested whether the local legalization of same-sex marriage was associated with changes in citizens' implicit and explicit biases. While antigay bias had been decreasing over time, following local same-sex marriage legalization antigay bias decreased at roughly double the rate, indicating that government legislation can inform attitudes even on religiously and politically entrenched positions. These results have important implications for those interested in intergroup bias, norms, and how policy shapes attitudes.

Author contributions: E.K.O., M.K.C., J.M.C., and E.H. designed research; E.K.O., M.K.C., and E.H. performed research; E.K.O. and E.H. analyzed data; and E.K.O., M.K.C., J.M.C., and E.H. wrote the paper.

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Data deposition: Datasets 1 and 2 have been deposited in the Open Science Framework, https://osf.io/prcd8/.

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It is noteworthy that state level antigay biases from the Project Implicit and ANES data were highly correlated [$r = 0.75$, $P < 0.001$, 95% CI (0.60, 0.85)], indicating that, despite sampling differences, the antigay bias captured by Project Implicit was highly correlated with a nationally representative estimate of explicit bias. Most importantly, we repeated model 1 with the ANES data across the three most recent time points corresponding with the passing of same sex marriage legislation: 2008, 2012, and 2016. Identical to models 1 and 2, a Year × Legalization interaction was present ($B = 1.1142$, SE = 0.3943, $P = 0.005$). Unlike the Project Implicit data that indicated prelegalization bias was declining slowly, simple slopes indicated that prelegalization warmth toward gay men and lesbians was stable over time ($B = 0.1466$, SE = 1.5062, $P = 0.813$). However, consistent with Project Implicit data and our hypotheses, warmth toward gay men and lesbians increased over time after legalization ($B = 2.3750$, SE = 0.6107, $P < 0.001$). See *SI Appendix*, Table S3, for the full model. Because the ANES is a US representative sample, these results provide evidence that our conclusions generalize beyond Project Implicit respondents.

Model 4: State- vs. Federal-Level Legalization. Although models 1–3 revealed that antigay prejudice was, on average, decreasing at a sharper rate across all US states following same sex marriage legalization, it is possible that this broad effect is concealing important moderators. For example, we have hypothesized that local norms influence individuals' attitudes. While 35 states and Washington, DC, passed same sex marriage legalization before federal legalization, 15 states did not. Therefore, any norms signaled by federal legalization would not be local within state and may have different implications for local antigay attitudes. To test this possibility, we coded states for whether legalization was first passed at the state or federal level and included a Year × Legalization × State Federal interaction testing whether the Year × Legalization effects reported above varied by whether legislation was passed at the state or federal level.

For implicit bias, this three-way interaction was significant ($B = 0.0088$, SE = 0.0016, $P < 0.001$), indicating that the pattern of change in bias over time depended on whether same sex marriage legalization happened as a result of local (i.e., state) or federal law. Before legalization, antigay bias was decreasing both in states that ultimately passed same sex marriage legislation ($B = -0.0051$, SE = 0.0004, $P < 0.001$) and in those that did not ($B = -0.0077$, SE = 0.0005, $P < 0.001$). For the states passing same sex marriage at the state level, the demonstrated pattern was identical to that evident in models 1–3 (Fig. 3). Bias decreased at roughly double the rate over time following legalization ($B = -0.0112$, SE = 0.0006, $P < 0.001$). In sharp contrast, for the 15 states that did not pass same sex marriage legalization locally, antigay bias increased over time following legalization ($B = 0.0215$, SE = 0.0065, $P < 0.001$). An identical pattern was present with explicit bias. Effects of interest explained 1 and 4.4% of the within state variance for implicit and explicit bias, respectively. See *SI Appendix*, Table S4, A and B, for full models. [Model 4 could not be replicated with the ANES dataset as the measurement over time was at a lower resolution (i.e., data were collected only around the presidential election in 2008, 2012, and 2016), which did not capture differences between states passing legalization at the state vs. federal level.] These results, while exploratory, suggest that the locality of legislation may be an important moderator in influencing the biases of local residents. We return to this issue in greater detail in *Discussion*.

Discussion

We find consistent evidence in support of the hypothesis that local government legislation informs changes in citizens' attitudes. Consistent with previous research (41), we find that both implicit and explicit antigay bias was decreasing or stable over time before same sex marriage legalization. However, following the passing of legislation perceived as supportive of this marginalized population, on average, antigay bias declined at a steeper rate. This conclusion converges with previous research finding that citizens of states passing state level legislation had the greatest decrease in antigay attitudes (27). Evidence is consistent across five different operationalizations and data from two distinct sources. The limited "multiverse" approach (39) that we pursued helps ensure that these conclusions are robust to unavoidable subjective researcher decisions. The manner in which same sex marriage legalization naturally unfolded across the United States, as a multiple group, time staggered quasi experimental design, mitigates many of the threats to causal conclusions typically associated with observational data (20–22).

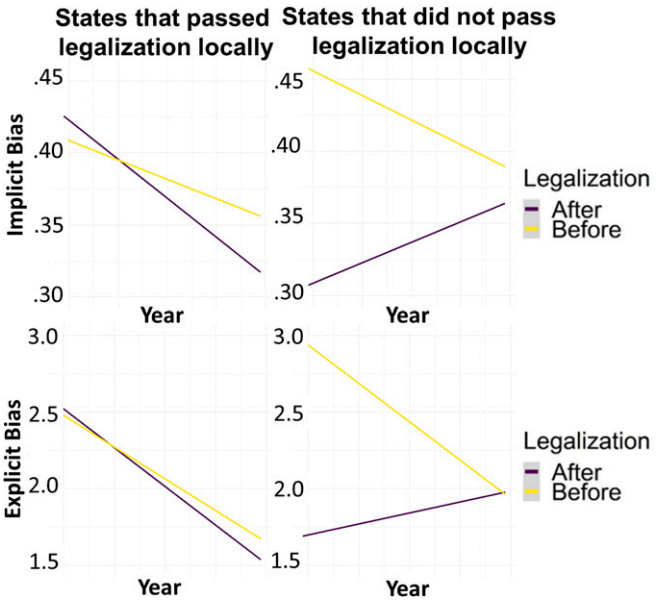

Fig. 3. The trends in implicit and explicit antigay bias over time, comparing the trend before and after same sex marriage legalization in states that passed same sex marriage legalization locally compared with states that did not pass same sex marriage legalization locally. The dates of these trends vary across different states, so they have been plotted on the same panels for purposes of comparison.

Results indicate that attitudes and legislation may be mutually reinforcing. More specifically, because results generally indicate that attitudes toward the gay community were improving in all states before legalization (although see *Model 3: Replication with a Nationally Representative Dataset*), evolving attitudes toward same sex marriage may have served as impetus and momentum for both state and federal legalization. These enacted legislations in turn strengthened and consolidated favorable attitudes toward lesbians and gay men.

Importantly, we find identical effects among both implicit and explicit measures of bias. The limited previous research on whether same sex marriage legislation was associated with changes in attitudes used self-reported measures that were susceptible to concerns regarding social desirability (28), especially so given the politically sensitive and controversial nature of the topic. That a similar pattern is evident among implicit measures, which are less susceptible to conscious control and social desirability, is important evidence supporting that government legislation is associated with true changes in the attitudes of its citizens. Traditionally, implicit and explicit biases at the individual level have been treated as weakly positively correlated but distinct phenomena (42), yet throughout all analyses here results were identical across both, and the correlation between

the two constructs was surprisingly high ($r = 0.88$, $P < 0.001$). Determining how regional biases may differ from individual level biases is beyond the scope of the present research, but future work might examine this discrepancy to better understand re gional biases. Here, examining both implicit and explicit biases revealed identical conclusions.

A critical moderator of this effect appears to be whether same sex marriage legalization was passed locally or at the federal level. In states that did not pass same sex marriage legalization locally, we find a reactive or "backlash" effect (27) such that federal legalization was associated with increased antigay bias over time, despite the decreasing trend in bias in these states before federal legalization. The specific factors driving this effect cannot be addressed by the present data. One possibility is that, even though attitudes were improving, a tipping point of local support had not yet been reached for the majority to accept the federal ruling. Research at the individual level suggests that the attention given the federal decision may have sharpened some respondents' sense of symbolic threat to their lifestyle and values (43), and this sense of threat could have exacerbated antigay biases among those individuals. Most of the 15 states that did not pass state legislation are those with generally stronger and more traditional social norms (44 46).

These increasingly positive attitudes in some states and in creasingly negative attitudes in others indicate that the federal legalization of same sex marriage may have prompted national group polarization on attitudes toward gay people. We have proposed that legislation signals majority norms, and this po larized result highlights the potential importance of the per ceived locality of that norm. Should legislation be perceived as imposed upon the local culture, a backlash effect might be expected. The analyses marshalled above provide tentative evidence that more localized policies may be more strongly associated with attitude change, perhaps because the norm is perceived as stronger and arising from a more local population.

One limitation of the present work concerns the representa tiveness of Project Implicit respondents. In general, these respon dents are unlikely to be representative of the North American population, and indeed, our comparisons in the present research reveal they are younger and more likely to be female. And yet a growing body of literature using this sample finds that it is pre dictive of meaningful population level behaviors. Thus far, these include outcomes such as being killed by police (36), mortality rates from cardiovascular disease (37, 38), segregation (35), and Google searches for racial slurs (35). These results collectively indicate that Project Implicit is tapping meaningful variation in the population, but the generalizability of these results was a concern. Accordingly, a strength of the present research is finding an identical pattern of results in a representative sample, the ANES dataset. That we find that antigay bias declines at a sharper rate following same sex marriage legalization in a representative sample strongly buttresses the conclusions of the present research.

Furthermore, it should be noted that the effects of same sex marriage legalization reported here are modest in size, with models explaining between 1 and 5% of the within state variance. In the field, smaller or similar effect sizes have been considered meaningful across diverse domains, including a baseball player's batting skill on their likelihood of getting a hit ($R^2 = 0.0033$) (47) or the daily use of aspirin on heart attacks ($R^2 = 0.0011$) (48). As others have pointed out, these seemingly small effect sizes may be societally meaningful when scaled across entire populations (49). See *SI Appendix* for more detailed contextualization of our effect sizes.

The broad conclusion of the present research that repre sentative governments can contribute to and/or intensify change in the attitudes of citizens by passing legislation has important implications. For example, research reviewing the effectiveness of bias interventions found limited effects, and no effect that persisted beyond several days (50). However, the current results suggest that perceived norms may evoke more persistent change. Additionally, we examine attitudes toward a sexual minority, which previous research has found to be particularly entrenched (24, 25), and the current results therefore provide a strong test of our hypotheses. In this case, attitudes toward minority groups became more positive, although government signaling of norms might increase prejudice as well. For example, recent research using a different theoretical lens has found increased xenophobic attitudes following Trump's election (16), which might be inter preted as signaling support of such attitudes. Furthermore, results might be extended toward other more malleable attitudes not involving social categories, such as toward littering or marijuana use. In addition, the amount of publicity any legislation receives may moderate these effects (51). For example, should legislation pass with little media attention or fanfare, the possibility that this legislation represents the attitudes of the majority will be less salient to citizens. Subsequently, the norm based model of legis lation changing attitudes would predict little change in citizens' attitudes. Finally, as government legislation may only be perceived as signaling "the will of the people" in representative governments, effects may be limited to such governing styles (i.e., not extending to citizens' of autocratic governments). In summary, our results evince that state and federal legalization was associated with changes in antigay bias, providing important evidence supporting the idea that government legislation can cause changes in the attitudes of its citizens regarding minority groups.

Materials and Methods

Source of Data.

Antigay bias. Measures of implicit and explicit antigay prejudice were obtained from Project Implicit (33). Implicit bias was represented by the IAT *d* score (52) from an IAT task requiring participants to respond to social targets (e.g., Gay, Straight) and attributes (e.g., Good, Bad) simultaneously by timed keyboard input. Explicit bias was calculated from thermometer items. Par ticipants had reported how warm they felt toward straight men, straight women, gay men, and lesbians on a 0 (coldest feelings) to 10 (warmest feelings) scale. Ratings of heterosexuals were averaged, and ratings of gay men and lesbians were averaged. Consistent with previous research (36 38), explicit bias was represented by the difference between rated warmth to ward heterosexuals and gay people. Greater positive values for both implicit and explicit biases thus reflected more positive attitudes toward hetero sexuals relative to the gay community.

Legalization. Same sex marriage legalization date was defined as the date on which state level institutions passed legalization locally. The earliest available data in the Project Implicit antigay dataset was 2005, after Vermont and Massachusetts had already passed forms of same sex marriage legalization. All data from these states were coded as post legalization. California initially enacted same sex legislation in 2008, but it was subsequently blocked 5 mo later. In 2010, legislation was again enacted legalizing same sex marriage. Thus, implicit and explicit responses from California were coded as post state legalization if they were performed on or after the 2010 date to be as conservative as possible. (See *SI Appendix* for analyses examining changes in California spe cifically.) In some states (e.g., Utah, Colorado, Oklahoma), we defined same sex marriage legalization as the date on which legislation defining marriage as "between woman and man" was ruled unconstitutional by state courts.

Time varying covariates. A number of demographic variables were included as controls. These variables varied by year. Employment was represented by 5 y estimates of state employment rates reported by the 2005 2016 American Community Survey (53). Education was represented by the percentage of state residents with a BA or equivalent degree (53). Population density was computed based on 2000 and 2010 census data (53). Socioeconomic status was represented for by 5 y estimates of mean household income (53).

Datasets. A number of different datasets with different exclusion criteria were created for different analyses. The antigay bias dataset had 949,664 respon dents, completed between 2005 and 2016. Across all datasets, participants ($M_{age} = 24.72$, SD = 10.8; 60% female, 32% male, 8% undisclosed) were in cluded only if they were US based, had state level geographic information included and either implicit or explicit data, and gender, age, and racial majority/minority status reported. All reported effects are robust to inclusion of participant level covariates, as models fully replicate when not included. In Dataset 1 (https://osf.io/prcd8/), analyses were restricted to participants who

EXHIBIT K

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OXFORD

Beyond Dissent and Compliance: Religious Decision Makers and Secular Law

Netta Barak-Corren*

ABSTRACT

Throughout history and across many societies, people have been facing conflicts be tween faith and the law. This article exposes and defines central dynamics of these con flicts by empirically investigating how educational leaders in religious communities tackle, reduce, and resolve conflicts with the law. Three main dynamics emerge: first, re ligious leaders frequently seek to harmonize law and religion by redefining the conflict around different themes, arguing that 'true religion' raises no conflict with the law or that 'real law' has been misinterpreted by the courts. Second, leaders withdraw religious normativity from conflict zones through nuanced distinctions of sphere and role. Third, religious leaders seek to restrain the reach of legal norms by constraining and reshaping state power. By highlighting the personal and institutional dimensions of conflict reduc tion and demonstrating the broader explanatory power of the theory, this article con tributes to our understanding of one of the most enduring forms of conflict in society.

1. INTRODUCTION

September 3, 2015, was an unusual day for Kentucky's small Rowan County. On this day, the County saw its clerk, Kim Davis, sent to jail for refusing to comply with a court order to issue marriage licences to gay couples. In a statement released through her attorneys, Ms Davis explained her defiant decision: 'I never imagined a day like this would come, where I would be asked to violate a central teaching of Scripture and of Jesus Himself regarding marriage', she wrote. 'It is not a light issue for me. It is a Heaven or Hell decision.'

Ms Davis is a contemporary controversial example of a more enduring and general phenomenon. Throughout history and across many societies, people have been facing conflicts revolving around myriad issues and beliefs between their faith and the law. While Davis' refusal to issue marriage licences is particularly extreme due to her

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position as an elected government official who took a public role and swore to uphold the Constitution, her narrative follows the same lines of justification of others who seek to defy secular law in the name of divine law. These conflicts divide societies and continually preoccupy the courts. Many of them have become known as instances in which avid believers defy the law and face litigation and possibly sanctions. Ms Davis is, therefore, just one link in an enduring chain of cases suggesting that when secular law conflicts with religious law, disobedience is inevitable. Or is it?

Commenting on Davis' refusal to issue marriage licences, one gay marriage activist said, 'If the big backlash and the mass resistance that our opponents promised is one clerk from a county of under 25,000 people, I think we're in really good shape.'[1] To anyone following the religious campaign against gay marriage, this observation must be striking. Indeed, despite vehement religious opposition to the legalization of same-sex marriage, only few clerks refused to issue marriage licences and none of them, except Davis, remained defiant to the point of civil contempt. How can that be? More specifically, if many religious individuals oppose the law yet choose not to defy it, what *do* they choose instead? How do they tackle the seemingly intractable conflict between law and religion? What explanations do they provide, and what behaviours lie in between disobedience and compliance with the law?

To answer these questions, this article empirically examines how religious people perceive and tackle conflicts between secular law and religion. I follow the influential research tradition on legal consciousness[2] to study the thoughts, feelings, and behaviours of people in everyday life, including their understanding and interpretation of their experiences, and the transformations that occur in their thinking. Though subjective, these individual accounts often shed light on social institutions and expose their structure.[3] In the present context, the object of these thoughts, feelings, and experiences is not exclusively the law, but also religion, and the strained relationship between the two. I, therefore, call this type of legal consciousness *conflict consciousness*.

To study religious conflict consciousness in situations of competing legal and religious norms, I conducted in-depth interviews with 41 religious educational leaders. Educational leaders, in particular, are an interesting population because, by virtue of their role, they instil values and norms and socialize the next generation into religion and the community.[4] At the same time, education is commonly regarded as a public good and is heavily regulated by secular law (in part for the same reasons, namely the societal interest in socializing children to become productive, law-abiding

1 Blinder, Alan, and Tamar Lewin. 'Clerk in Kentucky Chooses Jail over Deal on Same-Sex Marriage', *New York Times* (University of Chicago Press 3 September 2015).

2 Patricia Ewick and Susan S Silbey, *The Common Place of Law: Stories from Everyday Life* (University of Chicago Press 1998).

3 ibid; Elizabeth Hirsh and Christopher J Lyons, 'Perceiving Discrimination on the Job: Legal Consciousness, Workplace Context, and the Construction of Race Discrimination' (2010) 44 Law & Society Review 269.

4 Sarah Barringer Gordon, *The Spirit of the Law* (Harvard University Press 2010); Austin Sarat, *Legal Responses to Religious Practices in the United States* (Cambridge University Press 2012); Jeff Spinner-Halev, 'Education, Reconciliation and Nested Identities' (2003) 1 Theory and Research in Education 51; Nomi Maya Stolzenberg, '"He Drew a Circle That Shut Me Out": Assimilation, Indoctrination, and the Paradox of a Liberal Education' (1993) 106 Harvard Law Review 581; Robert M Cover, 'Foreword: Nomos and Narrative' (1983) 97 Harvard Law Review 4.

citizens). Because educational leaders' religious responsibilities are often coupled with legal ones, they are highly likely to experience norm conflicts and to experience navigating them in practice. Indeed, many conflicts between law and religion in the modern era revolve around education, from teaching religiously controversial subjects to religiously motivated discrimination in admissions and hiring.[5] Yet, as I suggest towards closure by returning to the Davis case, the insights stemming from this study may shed light on additional instances of conflict between law and religion.

The case study for this study and where the interviews were conducted is Israel, where there have been high-profile conflicts in recent years around gay acceptance, religion-based ethnic and pregnancy discrimination, and teaching curriculum, among other issues. The abundance and variety of conflicts, alongside their strong similarity to conflicts facing religious communities in other Western democracies, make Israel a highly relevant case study. Israel also offers several unique advantages for this type of research. First, it provides an opportunity to engage with a highly diverse religious society in a relatively small geographical area which is governed by one legal system, thus minimizing much of the demographic and legal variation that may differentiate religious communities in other countries. Second, Israel is a powerful case study because its dual identity as a Jewish and democratic state intensifies the conflict between secular and religious laws in political, legal, and social debates.[6] This intensity sharpens the issues at stake and can inform lawmakers and religious communities facing similar issues in other legal regimes.

Indeed, the study findings portray a rich and nuanced picture of conflicts between law and religion. Most educational leaders have personally experienced conflict with the law, many have opposed some forms of legality, and some have participated in breaking the law. At the same time, many leaders show a surprising inconsistency: they acknowledge the conflict between law and religion while virtually denying its existence. At face value, these views seem incoherent. A closer analysis, however, reveals a dynamic of cognitive dissonance[7] in which people seek to mitigate the conflict they experience between competing cognitions and norms. My findings show that in order to mitigate the conflict, religious leaders attempt to redraw the boundaries of law and/or religion in three related ways. I define these practices as redefinition, withdrawal, and restraint and show that each has substantial practical consequences. *Redefinition* involves altering the meaning of law and religion such that they do not conflict. *Withdrawal* involves forgoing religious claims with respect to certain people or realms, effectively evacuating religious normativity from some zones of conflict. *Restraint* involves seeking to impose limitations on the execution and enforcement of legal normativity (as exercised by judges, policemen, and other officials), in order to sustain religious authority.

These practices thwart the conventional paradigm of dissent/compliance. Rather than simply defying secular law or obeying it, religious leaders act as problem-solvers, redefining the meaning and scope of law and religion in ways that reduce their conflict.

5 Gordon (n 4); Cover (n 4).

6 Gila Stopler, 'Religious Establishment, Pluralism and Equality in Israel—Can the Circle be Squared?' (2013) 2 Oxford Journal of Law and Religion 150.

7 Leon Festinger, *A Theory of Cognitive Dissonance* (Stanford University Press 1962). See page 8 for a more detailed discussion.

The leaders' converging accounts reveal common institutional practices with significant implications. For example, educational leaders commonly restrict religious norms to the public sphere and turn a blind eye to private violations of religious norms. The resulting practice walks a fine line between deviance (in public) and compliance (in private). In yet another common practice, religious leaders seek to restrain the law's coercive normativity by substituting monetary sanctions for specific performance. In their view, this course provides a way of compliance even when they disagree with the law while simultaneously forming an effective outlet for dissent. I provide additional instances of redefinition, withdrawal, and restraint in the findings section.

The theoretic contribution of this research is three-fold. First, it nuances and enriches the legal discourse on conflicts between law and religion with empirical evidence on the lived experiences of central religious stakeholders, while mapping and theorizing the central themes that emerge from these experiences. The study thus offers the first grounded theory[8] of how individuals navigate conflict between contending legal and religious norms. The implications of this research are particularly illuminating, given the frequent assumption that the conflict between law and religion is intractable. While the ways in which religious leaders redraw normative boundaries and redefine the scope and meaning of law and religion offer no definitive solutions to the conflict; they provide directly applicable tools to reduce the conflict and may be used to generate more insights on how to cope with current conflicts between law and religion.

Second, this study extends the research on legal consciousness by focusing on conflicts and groups that have not been thoroughly studied before. Previous research on the legal consciousness of dissent primarily examined conscientious objectors from military service[9] and radical environmentalists,[10]but not people in official positions. The informants were individual activists who were already committed at the time of their interview to a course of conflict with the law. (Aviram, for example, interviewed activists in prison or shortly after their release.) Their situation and the timing of their interviews made these activists inherently more likely to provide justifications that undermine legality to justify their actions.[11] Their post-objection status also made them less likely to discuss ways to mitigate the conflict in advance. In contrast, the educational leaders in the present study were not activists, but leaders in official positions who experienced normative tensions of varying intensity as part of their job and on a repeated basis. By presenting their narratives and experiences, the present study sheds light on a form of norm conflict that was relatively unexplored to date.

It is also worth noting that previous studies of dissent did not engage religious populations. While the activists in these studies often justified dissent in the name of

8 As defined in Barney G Glaser and Anselm L Strauss, *The Discovery of Grounded Theory: Strategies for Qualitative Research* (Transaction Publishers 1967).

9 Hadar Aviram, 'When the Saints Go Marching In: Legal Consciousness and Prison Experiences of Conscientious Objectors to Military Service in Israel' in Laura-Beth Nielsen and Ben Fleury-Steiner (eds), *The New Civil Rights Research* (Dartmouth-Ashgate 2005).

10 Erik D Fritsvold, 'Under the Law: Legal Consciousness and Radical Environmental Activism' (2009) 34 Law & Social Inquiry 799; Simon Halliday and Bronwen Morgan, 'I Fought the Law and the Law Won? Legal Consciousness and the Critical Imagination' (2013) 66 Current Legal Problems 1.

11 Indeed, both Aviram (n 9) and Halliday and Morgan, ibid, found their informants feeling 'above the law'.

a higher moral law, *religion* was not their higher law. Instead, dissenters reasoned their actions, even in the context of a collective movement, as individualistic acts stemming from personal morality. In contrast to the religious leaders in the present study, they did not draw on a comprehensive, totalistic normative system like religion.[12] This difference is significant, because systems that aspire to encompass and explain the entirety of human existence inevitably include tensions and contradictions that are often expressly formulated and discussed. Indeed, many religious leaders discussed normative tensions *within* religion that they often brought to bear on the relationship between law and religion. Consequentially, their accounts nuance legal consciousness beyond previous accounts of the conflict between law and competing ideologies. These differences do not necessarily suggest that religion is special[13] or that there are no similarities between secular dissenters and religious dissenters.[14] But I believe that these differences help explain the novelty of the research and why it expands our accounts of the myriad relationships between individuals, communities, and legal institutions.

This article proceeds as follows: I begin with a brief background of law, religion, and education in Israel as well as a discussion of my methodology and sample. The results begin with a descriptive discussion of the type of conflicts that educational leaders experienced, and are then organized around the three themes I discovered: redefinition, withdrawal, and restraint. Following conclusion, I revisit the Davis example to examine the explanatory power and potential reach of the three-partite model I develop in this article. The results are nuanced and surprising, and show that the theoretical framework established in this article can potentially extend beyond the scope of conflicts studied in this article.

2. LAW, RELIGION, AND EDUCATION IN ISRAEL

The case study for the present research is Israel, where tensions between secular law and religion have occupied the public and the courts since the conception of the state. Israel is defined as the national home of the Jewish people and as a Jewish and democratic state. As such, it provides public funding to Jewish courts and schools (it also funds institutions of other religions, to some extent and with varying levels of supervision and autonomy). At the same time, Israeli law is largely secular and liberal, with deep common law roots and strong North American influences. All laws are generally binding for religious groups and individuals, with few religious exemptions.[15] Ultimately, conflicts between law and religion pervade the country much like they do in other liberal democracies.[16] As I noted earlier, due to Judaism's special

12 Aviram (n 9); Halliday and Morgan (n 10).

13 In the sense discussed in Steven G Gey, 'Why Is Religion Special: Reconsidering the Accomodation of Religion under the Religion Clauses of the First Amendment' (1990) 52 University of Pittsburgh Law Review 75; Andrew Koppelman, 'Is It Fair to Give Religion Special Treatment' (2006) University of Illinois Law Review 571.

14 Indeed, traces of 'above the law', see Aviram (n 9), and the classic schemas of legal consciousness—'before', 'with', and 'against the law', see Ewick and Silbey (n 2), occasionally surfaced in the data.

15 Shimon Shetreet, 'State and Religion: Funding of Religious Institutions—The Case of Israel in Comparative Perspective' (1999) 13 Notre Dame Journal of Law, Ethics & Public Policy 421.

16 See Lotem Perry-Hazan, 'Court-Led Educational Reforms in Political Third Rails: Lessons from the Litigation over Ultra-Religious Jewish Schools in Israel' (2015) 30 Journal of Education Policy 1. For a

Table 3 Common methods of redrawing normative boundaries

Redrawing Method	The object of redrawal	Incidence
Redefinition	Religion	44% (18/41)
	Law	22% (9/41)
Withdrawal	Sphere based	46% (19/41)
	Role based	39% (16/41)
Restraint	Sanctioning power	34% (14/41)
	Administrative Discretion	27% (11/41)

Note: Numbers do not sum up to 100% because some interviewees applied more than one method. Methods are ordered according to popularity.

apply to religious groups. The challenge, then, for restraint mechanisms is to reconcile the conflict between law and religion at the local level without compromising the rule of law more generally.

6. AFTERMATH

After five days in jail, Kim Davis returned defiant to her office. But things changed in her absence. First, her deputies assumed her role. 'I don't really want to, but I will follow the law,' said one deputy. 'I'm a preacher's daughter, and this is the hardest thing I've ever done in my life.'[72] Originally, Davis' deputies joined her in defying the law, but while she was in jail they began issuing marriage licences (all but one deputy, Davis' son).

The second change that occurred had to do with the marriage licence that Davis' office now issued. Instead of bearing Davis' name and title, it said 'pursuant to federal court order.' Davis told the media that '[U.S. District Judge David Bunning] indicated last week that he was willing to accept altered marriage licenses even though he was not certain of their validity,' Davis said. 'I, too, have great doubts whether the licenses issued under these conditions are even valid.'[73] The court order, however, reveals no such uncertainty. It states that the deputies have agreed under oath to issue the licences and that the plaintiffs did not question the new licences, and it orders that Davis shall not interfere, directly or indirectly, with their efforts.[74] Davis later clarified that she does not doubt the validity of the new licences.

Notwithstanding the fact that Davis unlike most of the educational leaders is a committed religious objector and that her conflict was not implicated in an official religious responsibility (her only office being public), the compromise achieved in her conflict demonstrates key features of the model I established in this article. Therefore, in this last section, I apply the model to the case and show that the central features and dynamics of the solution achieved in the Davis case can be readily understood as acts of redefinition, withdrawal, and restraint.

72 Blinder and Lewis (n 1).

73 Mariano Castillo and Kevin Conlon, 'Kim Davis stands ground, but same-sex couple get marriage license' *CNN* (14 September 2015) <http://www.cnn.com/2015/09/14/politics/kim-davis-same-sex-marriage-kentucky/> accessed 16 February 2017.

74 *Miller v Davis*, No 0:15-cv-00044-DLB, Doc 89 (ED Ky, 8 September 2015).

The first striking fact about the compromise is the extensive redefinition efforts that Davis initiated after being released from jail. These efforts were not merely cognitive or rhetorical, but entirely practical. Davis altered the *text* of the marriage licence in two significant ways. First, she omitted her name, thus escaping taint with same-sex marriage (or 'hell'). She could have left it at that, allowing her deputies to continue issuing the same licence with a different signature. But instead, she continued to redefine the very duty that conflicted with her beliefs, writing that the licence is issued pursuant to a *court* order rather than the law. Shifting responsibility to the court allowed her to maintain her position that her actions are in fact *legal*, 'protected under the First Amendment, the Kentucky Constitution, and in [*sic*] the Kentucky Religious Freedom Restoration Act.' Hence, similar to the religious educational leaders in my study that objected the law, she used the 'court' as a distancing mechanism that allowed her to place the blame for the conflict not with 'law', but with another entity. And, despite her objection and dissent, she still sought to avoid being marked as deviant and emphasized that the judge 'was willing to accept' her solution. Her claim that the modified licences are potentially invalid similarly places her on the same footing as the objecting educational leaders in distinguishing 'true law' from its mistaken application, for which the court is to blame. We see, then, the power of redefinition techniques in facilitating practical compromise, by enabling religious objectors to achieve cognitive consonance.

A subtle yet crucial component of the Davis compromise involves withdrawal, in a directly similar format to the one discussed by the religious educational leaders. Davis was held in contempt and jailed because she refused to issue marriage licences, directly or through her deputies. But, after her release she agreed not to prevent her deputies from issuing marriage licences to gay couples. Remarkably, then, the escalation of the conflict encouraged Davis to adopt a *role-based* withdrawal: 'I am forced to fashion a remedy that reconciles my conscience with Judge Bunning's order,' she told reporters outside the courthouse. 'I love my deputy clerks and I hate that they have been caught in the middle. If any of them feels that they must issue an unauthorized license to avoid being thrown in jail, I understand their tough choice and I will take no action against them.'

By withdrawing religious normativity from her deputies relieving them from the requirement to follow her beliefs Davis practically solved the conflict. The analogy to the teacher/student withdrawal is striking. The educational leaders and the clerk, professed agents of religious normativity, reduced conflict with the law by withdrawing religious normativity from those whom they viewed as subjects, rather than agents, of religious normativity. While Davis did not initiate this compromise, her statements indicate that she was able to accept that her deputies would continue to issue marriage licences. Being able to redefine the conflict and stand firm on narrower 'sovereign' religious grounds enabled Davis to come to terms with her legal obligations and end the conflict, to the general satisfaction of gay couples, the court, and state officials.[75]

75 Michael Muskal 'Kentucky clerk Kim Davis allows office to issue "unauthorized" marriage license' The Los Angeles Times (14 September 2015) <http://www.latimes.com/nation/la-na-will-she-or-not-kim-davis-20150914-story.html> accessed 16 February 2017.

The case also surfaces interesting, yet underdeveloped, elements of restraint. Davis sought restraint by repeatedly asking for a religious accommodation, first from the court and then from the Kentucky legislator. Clearly, these requests were not initially granted. Under federal law, the court had the power to impose fines, imprisonment, or both on Davis, once it found her in contempt of the court (18 USC section 401). As the court placed Davis in custody, the potential consequences of substituting confinement for fines remained obscured. Notably, plaintiffs asked to fine Davis, not to imprison her, but the judge believed that fines would not bring about compliance. Was he right?

One specific concern that Judge Bunning reportedly noted in the courtroom is that others might raise money to pay the penalty on Davis' behalf. Some educational leaders made similar comments regarding monetary sanctions, but in general, they believed in the power of fines to bring about compliance. This raises an interesting question: were courts (or agencies) to accommodate the religious demand at a milder level of legality (monetary sanctions in lieu of compliance), would it reconcile conflict or create a cascade of second-order problems?

At least when it comes to substituting monetary sanctions for performance, there are reasons to suspect that restraint would not reconcile conflict. Substantial evidence indicates that monetary sanctions often have the opposite effect: they reduce intrinsic commitments to norms by making norms appear as ordinary transactions.[76] Indeed, many religious objectors referred to monetary penalties as simple prices (eg 'this is not a punishment, this is an outcome'). In such cases, non-compliance might transform into a commodity that people can purchase, and the negative impact of judicial restraint would outweigh its advantages.

Yet monetary penalties are not the only way to restrain legality. While Judge Bunning imprisoned Davis until she complied with the law, he also decided to release her after a short five days in jail without another attempt to secure her compliance. Instead, the judge placed the burden of compliance on other individuals — the deputies — effectively releasing Davis from the need to issue marriage licences herself. This belated form of restraint towards Davis, which expands the examples in my data, raises a host of interesting questions. First, was it effective? The conflict does seem to have worn off, but is it due to the (post hoc) judicial restraint or to the (original) lack thereof? As Davis apparently refused to accept a withdrawal solution prior to her imprisonment, but accepted it afterwards, *lack of restraint* seems to play a determinative role in the outcome. Indeed, Davis' acts of both redefinition and withdrawal followed punishment, raising deep questions about the role of punishment — albeit short and symbolic — in enabling compromise (the Davis case, in that sense, joins the Immanuel case, in which compromise was only achieved after segregating parents were sent to jail in contempt of the court, what became a highly criticized and controversial judicial decision). In the present case, given that the court was willing to allow Davis some narrow zone of disobedience in any event, it appears that

76 Samuel Bowles and Sandra Polanía-Reyes, 'Economic Incentives and Social Preferences: Substitutes or Complements?' (2012) 50 Journal of Economic Literature 368; Uri Gneezy, Stephan Meier, and Pedro Rey-Biel, 'When and Why Incentives (Don't) Work to Modify Behavior' (2011) 25 The Journal of Economic Perspectives 191; Uri Gneezy and Aldo Rustichini, 'Fine Is a Price, A' (2000) 29 Journal of Legal Studies 1.

restraint, too, played an important role in the eventual compromise and further facilitated acts of redefinition and withdrawal.

The analysis of the solution achieved in Davis' conflict shows that the compromise's building blocks are redefinition, withdrawal, and restraint. These features enabled Davis to redraw the boundaries between her legal and religious commitments, even after her dissent and jail punishment, and allowed her to eventually comply with narrower versions of both. Even more importantly, while redefinition, withdrawal, and restraint played a dominant role for Davis, they also facilitated the compromise between law and religion at an institutional and general level, enabling the court to craft a lasting relief for the couples harmed by Davis' disobedience, and to release her deputies from the obligation to follow beliefs that they did not share with the same forcefulness. Indeed, no party saw its claims answered in full. Davis has won a personal freedom to distance herself from gay marriage, but she was not able to impose her beliefs as public policy. Gay couples were able to marry in Rowan County, but they did not receive the same licences as heterosexual couples, as Davis was not signed on theirs. But the material interests of all parties were safeguarded, and a balance was struck. This compromise, as well as the many others I discussed in this article around the inclusion of gay and unmarried pregnant individuals, teaching curriculums, and other issues invite lawmakers, courts, and lawyers, as well as religious objectors themselves, to think imaginatively and creatively about the resources they currently have to advance compromises between law and religion and reduce the intensity of this incredibly important and divisive conflict.

***

Conflicts between law and religion can be agonizing for religious individuals, harmful for third parties, and divisive for society as a whole. Indeed, they are one of the most enduring and pervasive forms of social conflict. This article presented new evidence on how leaders in religious communities perceive, navigate, and resolve these conflicts on various levels. Furthermore, this article offers a new framework to understand these conflicts, by defining and examining their dynamics, from the cognitive dissonance that conflict entails to the nuanced ways in which key stakeholders seek to resolve it.

The discourse on conflicts between law and religion typically assumes that religious individuals would see the conflict in black and white terms and engage it with fervent piety. Contrary to these predictions, most of the religious leaders in this study across levels of piety and ideological positions demonstrate conflict aversion. Rather than defying or obeying the law, religious individuals in key normative positions are primarily occupied with redrawing the boundaries of law and religion to reduce and avoid conflict. They redefine the meaning of these normative systems to claim that they are, after all, not in conflict. They withdraw religious normativity to reduce its conflict with legality. And they seek to restrain legality to reduce its conflict with religion.

Beyond insight into the lived experiences of individuals, my findings reveal the institutional mechanisms of conflict reduction and reconciliation mechanisms that are already at work. The detailed cases throughout the article demonstrate the

practical usefulness of these mechanisms, as well as some of their limitations. The analysis of the Davis case further confirms the explanatory power of the model as well as its practical usefulness in reducing conflict between law and religion. Despite the apparent differences in role and dilemma between the state-elected county clerk and the religious educational leaders, reading the case through the prism of redefinition, withdrawal, and restraint elucidates under-explored aspects of the case and suggests that this framework can help mitigate conflicts between law and religion, even beyond the realm of education.

METHODOLOGICAL APPENDIX

A. Participants

The interview sample included 41 religious educational leaders representing a wide range of teaching positions, religious affiliations, and career stages (Table 1 summarizes the sample). Due to the relatively high insularity of the religious populations participating in the study, and the sensitive topics that were meant to come up in the discussion, I made initial recruitment through various referents, unrelated to me and independent from each other. Chain-referral sampling was then used to build up the sample, overcoming barriers of trust and access.[77]

B. Design

A semi-structured interview instrument was developed to identify the key conflicts, considerations, and strategies used by the educational leaders to cope with conflicts between law and religion in their field. Most interviews were recorded and transcribed following participants' approval. Some interviewees, however, opted not to be recorded; in these cases, I transcribed their comments in real time. Interviewee consent was given verbally and each interviewee was informed on the confidentiality of their response.[78] I supplemented the interview data by collecting documents and publications issued by religious schools and their administrations, and by closely following religious newspapers and websites.

Each interview consisted of two parts. The first part explored whether participants had personally experienced tension between competing normative commitments throughout their career. As the stories unfolded, I asked the educational leaders to describe the origin of these tensions, their choices, and the reasoning behind these choices. I then proceeded to probe participants' intuitions regarding two recent cases that contrasted the religious freedom of schools with Israeli anti-discrimination law by reading excerpts from the court decisions aloud to the participants. Each case took place in either a national- or an ultra-orthodox school (the two communities from which educational leaders were drawn). As such, the cases were directly

77 Patrick Biernacki and Dan Waldorf, 'Snowball Sampling: Problems and Techniques of Chain Referral Sampling' (1981) 10 Sociological Methods & Research 141; Janice Penrod and others, 'A Discussion of Chain Referral as a Method of Sampling Hard-to-Reach Populations' (2003) 14 Journal of Transcultural Nursing 100.

78 The study was approved by the Harvard IRB (#F24214-101) and by the Israeli Ministry of Education (file 7693(y)/827).

EXHIBIT L

UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION

CHELSEY NELSON PHOTOGRAPHY LLC and CHELSEY NELSON, **Plaintiffs,** **v.** **LOUISVILLE/JEFFERSON COUNTY METRO GOVERNMENT, et al.,** **Defendants.**	**Case No. 3:19-cv-851-BJB-CHL**

DEFENDANTS' OBJECTIONS AND RESPONSES TO PLAINTIFFS' FIRST SET OF INTERROGATORIES

Defendants Louisville/Jefferson County Metro Government, Louisville Metro Human Relations Commission – Enforcement, Louisville Metro Human Relations Commission – Advocacy, Kendall Boyd, in his official capacity as (former) Executive Director of the HRC, Marie Dever, Kevin Delahanty, Charles Lanier, Sr., Laila Ramey (former member), William Sutter, Ibrahim Syed, and Leonard Thomas, in their official capacities as members of the Louisville Metro Human Relations Commission-Enforcement (collectively, "Defendants"), by counsel, pursuant to Federal Rules of Civil Procedure 26 and 33, hereby provide their objections and answers to the First Set of Interrogatories served by the Plaintiffs Chelsey Nelson Photography LLC and Chelsey Nelson (collectively, "Plaintiffs" or "Chelsey Nelson"), as follows:

DEFINITIONS USED HEREIN

1. The term "Commission" refers to the Louisville Metro Human Relations Commission or its authorized representative. As the context requires, "Commission" may refer to

Defendants object to this Interrogatory as seeking information that is irrelevant to adjudicating the merits of this dispute and not likely to lead to the discovery of admissible evidence. Subject to that objection, Defendants do not contend that there are no wedding photographers within the geographic boundaries of the Louisville/Jefferson County Metro Government who are willing to provide services to same-sex couples. Defendants further state that if wedding photographers are permitted to discriminate against same-sex couples, the level of access to this service would be inferior to that available to opposite-sex couples.

7. Do you contend that any person living within the geographic boundaries of the Louisville/Jefferson County Metro Government has ever been denied access to wedding photography for a same-sex wedding photography? Identify all material facts that support your contention.

Objection/Answer:

Defendants object to this Interrogatory as seeking information that is irrelevant and not likely to lead to the discovery of admissible evidence and to adjudicating the merits of this dispute. Subject to that objection, the Commission is not aware of any specific instance on which a person has been denied access to wedding photography services on the basis of his or her sexual orientation; however, Defendants do not believe that lack of knowledge of a specific instance of such discrimination proves that this has never happened. Moreover, Defendants contend that the Public Accommodations Provision guarantees equal access to this service by same-sex couples, which would not exist if all such service providers are not required to comply with the Public Accommodations Provision.

17. Do you contend that the least restrictive means to achieve any government interest is to require Chelsey Nelson Photography LLC and Chelsey Nelson to write blogs celebrating same-sex weddings as part of her paid photography services when she already writes blogs celebrating opposite-sex weddings as part of her paid photography services? If so, identify all material facts that support your contention, including all other alternative means you considered, when you considered those alternative means, and why you concluded those alternative means were ineffective.

Answer:

See Answer to Interrogatory No. 15.

Respectfully submitted,

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Counsel for Defendants

VERIFICATION

I, Kendall Boyd, believe, based on a reasonable inquiry, that the foregoing answers to interrogatories are true and correct to the best of my knowledge, information and belief but not necessarily fully of my own knowledge and so verify under penalty of perjury.

January 25, 2021

/s/ Kendall Boyd
Kendall Boyd

EXHIBIT M

UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION

Case No. 3-19-CV-00851-BJB-CHL

CHELSEY NELSON PHOTOGRAPHY, LLC
and CHELSEY NELSON, PLAINTIFFS

v.

LOUISVILLE/JEFFERSON COUNTY METRO
GOVERNMENT, et al., DEFENDANTS

DEPONENT: KENDALL BOYD, 30(b)(6) REPRESENTATIVE

DATE: MAY 25, 2021

COURT REPORTER: JESSICA TAYLOR ROSS

TAYLOR COURT REPORTING KENTUCKY
2901 SIX MILE LANE
LOUISVILLE, KENTUCKY 40220

KENDALL BOYD

MAY 25, 2021

A. I do not know that.

Q. Got it. Let's see here. Now, the injunction in this lawsuit has been in place for, I think, roughly about a year. Since that injunction went into place, has Metro seen an increase in complaints on the basis of discrimination?

MS. HINKLE: Objection to form.

A. I would have to do a comparison of number of complaints received this -- during this period as compared to prior to the injunction that was put in place.

BY MR. SCRUGGS:

Q. So you don't know?

A. No.

MR. SCRUGGS: Let me take another 10-minute break, and then we can come back and go for some more. All right?

THE COURT REPORTER: We're off the record.

* * *

(Off the record.)

* * *

will go off.

* * *

(Witness Excused.)

* * *

STATE OF KENTUCKY)
) SS.
COUNTY OF JEFFERSON)

I, JESSICA TAYLOR ROSS, a Notary Public within and for the State at Large, do hereby certify that the foregoing deposition was taken before me, via Zoom, at the time and for the purpose in the caption stated; that the witness was first duly sworn to tell the truth, the whole truth and nothing but the truth; that the deposition was reduced to digital shorthand and recorded by me in the presence of the witness; that the foregoing is a full, true and correct transcript of my digital notes and recording; that there was no request that the witness read and sign this deposition; that the appearances were as stated in the caption.

WITNESS MY SIGNATURE this 26th day of May, 2021.

My commission expires July 21, 2022.

/s/ Jessica T. Ross
JESSICA TAYLOR ROSS
Court Reporter
Notary Public, State At Large
Notary ID 602031

PG/lt

EXHIBIT N

UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION

Case No. 3-19-CV-00851-BJB-CHL

CHELSEY NELSON PHOTOGRAPHY, LLC
and CHELSEY NELSON, PLAINTIFFS

v.

LOUISVILLE/JEFFERSON COUNTY METRO
GOVERNMENT, et al., DEFENDANTS

DEPONENT: VERNA GOATLEY

DATE: MAY 27, 2021

COURT REPORTER: JESSICA TAYLOR ROSS

TAYLOR COURT REPORTING KENTUCKY
2901 SIX MILE LANE
LOUISVILLE, KENTUCKY 40220

VERNA GOATLEY

MAY 27, 2021

Oh, that must have been an echo. I'm sorry.

Okay. Ms. Goatley, since the injunction went into place in this case in August 2020, are you aware of any increase in complaints filed with the Commission?

A. I'm not aware of the number of complaints before or after the filing of this -- of the numbers. I'd have to look at our records to see.

Q. On -- well, at the very beginning of the deposition today, you -- you mentioned how when you were the assistant director, you were involved initially with the certification program of the city.

A. Correct.

Q. And then, that's still part of your responsibilities as the executive director.

Right?

A. Correct.

Q. So would you kind of explain what that process is, again, please?

A. If a business owner is -- if a business is owned and operated 51 percent or more by a woman, an ethnic minority, a disabled person or persons, or an LGBT person, then we

STATE OF KENTUCKY)
) SS.
COUNTY OF JEFFERSON)

I, JESSICA TAYLOR ROSS, a Notary Public within and for the State at Large, do hereby certify that the foregoing deposition was taken before me, via Zoom, at the time and for the purpose in the caption stated; that the witness was first duly sworn to tell the truth, the whole truth and nothing but the truth; that the deposition was reduced to digital shorthand and recorded by me in the presence of the witness; that the foregoing is a full, true and correct transcript of my digital notes and recording; that there was no request that the witness read and sign this deposition; that the appearances were as stated in the caption.

WITNESS MY SIGNATURE this 28th day of May, 2021.

My commission expires July 21, 2022.

/s/ Jessica T. Ross
JESSICA TAYLOR ROSS
Court Reporter
Notary Public, State At Large
Notary ID 602031

PG/lt

EXHIBIT O

IN THE UNITED STATES DISTRICT COURT
FOR THE WESTERN DISTRICT OF TEXAS
AUSTIN DIVISION

U.S. Pastor Council, *Plaintiff*, v. City of Austin; Steve Adler, in his official Capacity as Mayor of the City of Austin; Sareta Davis, in her official capacity as Chair of the Austin Human Rights Commission, *Defendants*.	§ § § § § § § § § § § §	Case No. 1:18-cv-849-RP

DEFENDANTS' MOTION TO DISMISS PLAINTIFF'S COMPLAINT

RESPECTFULLY SUBMITTED,

ANNE L. MORGAN, CITY ATTORNEY
MEGHAN L. RILEY, CHIEF, LITIGATION

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ATTORNEY FOR DEFENDANTS

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DEFENDANTS' MOTION TO DISMISS PLAINTIFF'S COMPLAINT

TO THE HONORABLE U.S. DISTRICT JUDGE ROBERT L. PITMAN:

Defendants the City of Austin, and Steve Adler, in his official Capacity as Mayor of the City of Austin (together "City"),[1] and request that the Court dismiss Plaintiff's Complaint, as authorized under Federal Rule of Civil Procedure 12(b). In support, the City would show the Court the following.

I. SUMMARY OF MOTION

This lawsuit presents a hypothetical controversy in search of a real-world dispute. There is none. Plaintiff U.S. Pastor Council, a Houston a nonprofit corporation, sued the City of Austin over its employment discrimination ordinance ("Ordinance"). Plaintiff does not purport to have any employees in Austin and is not subject to Austin's Ordinance. Nevertheless, the Complaint charges the Ordinance is unconstitutional on its face because it allegedly forces a group of unidentified Austin churches to hire as employees practicing homosexuals, transgendered individuals and women, none of whom the Austin churches allegedly would ever hire as employees, owing to the religious beliefs of those churches on matters of sexuality and gender. The provisions of the Ordinance being challenged have been on the books for more than a decade. But there is no allegation the Ordinance has been enforced, or is about to be enforced, against any of the unnamed Austin churches and no allegation that the Ordinance has in fact restricted the free exercise of their religion.

The Complaint presents no live, justiciable controversy requiring adjudication. Plaintiff has no standing, either on its own behalf or on behalf of the unnamed Austin churches alleged to be members of the organization. The claims alleged are speculative, hypothetical, unripe and

[1] Sereta Davis has not been served with summons.

unlikely to manifest themselves. There are no allegations that the unnamed Austin churches are even covered by the Ordinance. Even if they were, the ministerial exception exempts a church's hiring decisions regarding its clergy. All of Plaintiff's claims should be dismissed, or in the alternative, partially dismissed, pursuant to Federal Rule of Civil Procedure 12(b).

II. BACKGROUND

A. The Plaintiff

According to the Complaint, the U.S Pastor Council is a nonprofit corporation headquartered in Houston. The corporation allegedly has around 1,000 member churches "including 25 in the city of Austin". *Complaint* ¶3. Beyond those bare facts, the pleading alleges nothing more about the Plaintiff, its organizational purpose, or how its employment practices are impacted by Austin's Ordinance. The Complaint alleges Plaintiff's Austin-based member churches, none of whom are identified, "rely on the Bible . . . for religious and moral guidance" and for that reason "will not hire practicing homosexuals or transgendered people as clergy." *Id*.**,** ¶10. The Complaint further alleges that because "many of these member churches" believe the Bible forbids a woman from serving as a senior pastor, they will not hire women for that job. *Id*., ¶12.[2]

B. The Ordinance

Employment discrimination was banned by the Austin City Council more than four decades ago in 1975. A copy of the July 1975 Ordinance No. 75 0710-A, which prohibited employment discrimination against individuals based on several protected classes, is attached to

[2]The Complaint does not identify which of the unnamed 25 Austin member churches, if any, refuse to hire women for the job of senior pastor. Apart from the referenced allegation, the only other reference in the Complaint to objections by churches to hiring women is the claim that the Ordinance "forbids [a Catholic church] from [excluding] Catholic women . . . from the priesthood." *Complaint*, ¶15. This hypothetical statement is not a factual allegation, but a legal conclusion, and a mistaken one at that. The Complaint's conclusory statement ignores the ministerial exception, discussed below.

this Motion as **Ex. 1**. A decade earlier, the federal government led the way with passage of the landmark Civil Rights Act of 1964. That Act outlawed, among other things, employment discrimination based on race, color, religion, sex or national origin, in the section the Act commonly referred to as Title VII. 42 U.S.C. §2000e-2. Austin modeled its 1975 Ordinance on the protections outlined in Title VII and extended the protections against employment discrimination to cover an individual's sexual orientation, age and "physical handicap."[3] A 2004 amendment to the Ordinance prohibited employment discrimination based on an individual's gender identity. A copy of Ordinance No. 040610-7 is attached as **Ex. 2**. In 1983, the State of Texas passed the Texas Commission on Human Rights Act, which was intended to provide rights and remedies against employment discrimination that are substantially equivalent to the protections established under federal laws. The Act is codified in Chapter 21 of the Texas Labor Code.

The policy underlying Austin's employment anti-discrimination Ordinance is "to bring about through fair, orderly and lawful procedures, the opportunity for each person to obtain employment without regard to race, color, religion, sex, sexual orientation, gender identity, national origin, age or disability." *Austin City Code* §5-3-1(A)[4]. In passing the Ordinance, the Austin City Council recognized "the inalienable rights of each individual to work to earn wages and obtain a share of the wealth of this City through gainful employment" and that employment discrimination against individuals in Austin, based on the classes specified, "is detrimental to the

[3] The federal government banned employment discrimination based on age with passage of the Age Discrimination in Employment Act of 1967 (ADEA). 29 U.S.C. §261, *et seq*. More than twenty years later in 1990, the federal government banned employment discrimination based on an individual's disability with passage of the Americans with Disabilities Act (ADA). 42 U.S.C. §12101, *et seq*. The ADA came 15 years after the City of Austin banned employment discrimination based on an individual's physical handicap. **Ex. 1**.

[4] Chapter 5-3 of the Austin City Code, which includes the portions of the Ordinance being challenged, is attached to the Complaint as "Exhibit 1".

health, safety and welfare of the inhabitants of the City." *Id.*, §5-3-1(B). To that end, the Ordinance provides:

> (A) An employer may not:
>
> (1) Fail or refuse to hire or discharge any individual, or otherwise discriminate against an individual with respect to compensation, terms, conditions, or privileges of employment, based on the individual's race, color, religion, sex, sexual orientation, gender identity, national origin, age, or disability; or
>
> (2) Limit, segregate, or classify an employee or applicant for employment in a way which would deprive or tend to deprive an individual of employment opportunities or otherwise adversely affect the individual's status as an employee, based on the individual's race, color, religion, sex, sexual orientation, gender identity, national origin, age, or disability. *Id.*, §5-3-4.

These prohibitions mirror those discriminatory employment practices barred by state and federal law.[5] The texts of Title VII and Texas Labor Code Ch. 21 do not explicitly list "sexual orientation" or "gender identity" as protected classes.[6] But the Equal Employment Opportunity Commission (EEOC), the federal agency charged with implementing and enforcing Title VII, has determined

---

[5] Texas Labor Code §21.051 provides.

> Discrimination by Employer. An employer commits an unlawful employment practice if because of race, color, disability, religion, sex, national origin, or age the employer: (1) fails or refuses to hire an individual, discharges an individual, or discriminates in any other manner against an individual in connection with compensation or the terms, conditions, or privileges of employment; or (2) limits, segregates, or classifies an employee or applicant for employment in a manner that would deprive or tend to deprive an individual of any employment opportunity or adversely affect in any other manner the status of an employee.

Title VII provides:

> It shall be an unlawful employment practice for an employer (1) to fail or refuse to hire or to discharge any individual, or otherwise to discriminate against any individual with respect to his compensation, terms, conditions, or privileges or employment, because of such individual's race, color, religion, sex, or national origin; or (2) to limit, segregate, or classify his employees or applicants for employment in any way which would deprive any individual of employment opportunities or otherwise adversely affect his status as an employee, because of such individual's race, color, religion, sex or national origin. 42 U.S.C. §2000e-2(a).

As noted, the ADEA and the ADA also prohibit employment discrimination based on age and disability.

[6] *See Brandon v. Sage Corp.*, 808 F.3d 266, 270 n.2 (5th Cir. 2015)(Title VII does not cover sexual orientation in its plain terms.); but *see Wittmer v. Phillips 66 Company*, 304 F.Supp. 3d 627, 633 (S.D. Tex. 2018)(Noting that several circuits have recently expanded Title VII protection to include discrimination based on transgender status and sexual orientation and finding that authority "persuasive").

that employment discrimination against individuals on those grounds under some circumstances is covered by Title VII.[7]

Like Title VII and the state law, the Austin Ordinance establishes a complaint procedure to address violations. An individual who believes there has been a violation of the Ordinance may submit a timely, written charge of discrimination to the City's Equal Employment/Fair Housing Office ("EEFH") for investigation. *Austin City Code* §5-3-6. Before proceeding with an investigation, the EEFH Office must determine that the charge adequately describes a violation of the Ordinance or federal and state employment discrimination laws, and notify the complainant if it does not. *Id.*, §5-3-7. If the EEFH Office accepts the charge, an investigator conducts an investigation to determine whether there is "reasonable cause to believe a charge is true." *Id.*, §5-3-9. If the EEFH Office finds reasonable cause, the Office will attempt to resolve the alleged violation through a conciliation agreement between the complainant and respondent. *Id.*, §5-3-12(A). If no conciliation agreement is reached, the EEFH Office may refer a case to the City Attorney for prosecution in municipal court, "or for other civil prosecution as authorized by Chapter 21 of the Texas Labor Code." *Id.*, 5-3-12(D). Unlike the federal and state laws, the Ordinance does not establish a private cause of action that may be brought by an aggrieved individual.

C. Plaintiff's claims

The Complaint alleges Plaintiff's member churches in Austin "require church employees to live according to the Bible's teachings on matters of sexuality and gender" and for that reason

[7]*See e.g. Macy v. Dep't of Justice*, E.E.O.C. Appeal No. 0120120821 (April 20, 2012)(Discrimination against an individual because that person is transgender is discrimination because of sex under Title VII.); *see also Complainant v. Foxx*, E.E.O.C. Appeal No. 0120133080 (July 16, 2015)(Discrimination based on sexual orientation can be brought under Title VII even without any further showing of sex stereotyping.)

they "will not consider practicing homosexuals or transgendered people for any type of church employment." *Id.*, ¶11.

The Complaint alleges the Ordinance is facially invalid because it does not specifically "exempt church hiring decisions from its anti-discrimination laws." *Complaint*, ¶16-18. Based on these allegations, the Complaint asserts three causes of action. First, Plaintiff makes a facial constitutional challenge to the Ordinance under 42 U.S.C. §1983, alleging the Ordinance violates the Free Exercise Clause of the First Amendment of the U.S. Constitution. Relatedly, Plaintiff alleges the Ordinance on its face violates art. 1, sec. 6 of the Texas Constitution's Bill of Rights. That section, titled "Freedom of Worship", provides that all men have the "right to worship Almighty God according to the dictates of their own consciences" and prohibits a human authority from controlling or interfering with the rights of one's conscience in religious matters. Tex. Const. Art. 1, §6. The Complaint does not specify how the Ordinance violates its rights under the Texas Constitution, but presumably Plaintiff is claiming that the free exercise of religion by its Austin member churches is impinged.[8] Third, Plaintiff makes a claim under the Texas Religious Freedom Restoration Act (TRFRA)[9], alleging "Austin's refusal to exempt church hiring decisions from its anti-discrimination laws violates" the Act. *Complaint*, ¶18.

The Complaint seeks injunctive and declaratory relief, specifically that the unnamed Austin member churches:

- have the right under the federal and state constitutions "to exclude practicing homosexuals and transgendered people as clergy and church employees" notwithstanding any federal, state, or local anti-discrimination laws to the contrary; *Complaint*, P. 6-7, a & b.

[8] The Texas and federal constitutional provisions regarding the exercise of religion are ordinarily treated as coextensive. *See Tilton v. Marshall*, 925 S.W.2d 672, 677 n. 6 (Tex. 1996)(Noting similarity between the free exercise guarantees under state and federal constitutions).

[9] Tex. Civ. Prac. & Rem. Code Chapter 110 .

- have a protected right under the TRFRA to exclude practicing homosexuals and transgendered people as clergy and church employees" notwithstanding any local anti-discrimination ordinance; *Id.*, P. 7, c.

- have a right under the federal and state constitutions "to hire only men as clergy" notwithstanding any federal, state, or local anti-discrimination laws to the contrary; *Id.*, P. 6-7, d & e.

- have a protected right under the TRFRA "to hire only men as clergy" notwithstanding any local anti-discrimination ordinance; *Id.*, P. 7, f.

III. ARGUMENT AND AUTHORITY

A. The Complaint fails to plausibly plead standing.

1. The standard of review for standing.

Standing is a constitutionally-mandated component of subject matter jurisdiction. The requirement that a party have "'standing is an essential and unchanging part of the case-or-controversy requirement of Article III." *Nat'l Fed'n of the Blind of Texas, Inc. v. Abbott*, 647 F.3d 202, 208–09 (5th Cir. 2011) (quoting *Davis v. FES*, 554 U.S. 724 (2008)). The party invoking federal jurisdiction has the burden of establishing the elements of standing. *Lujan v. Defenders of Wildlife*, 504 U.S. 555, 561 (1992). "[E]ach element must be supported in the same way as any other matter on which the plaintiff bears the burden of proof, *i.e.*, with the manner and degree of evidence required at the successive stages of the litigation." *Id.* A court must dismiss a case under Rule 12(b)(1) for lack of subject matter jurisdiction if it lacks the statutory or constitutional power to adjudicate the case. *Home Builders Ass'n of Miss. v. City of Madison*, 143 F.3d 1006, 1010 (5th Cir. 1998).

The "irreducible constitutional minimum of standing" consists of three elements: (1) an injury in fact which is concrete and particularized and actual or imminent, not conjectural or hypothetical; (2) a causal connection between the injury and the complained-of conduct—that is, that is fairly

traceable to the challenged action of the defendant; and (3) it is likely, as opposed to merely speculative, that the injury will be redressed by a favorable decision. *Lujan* 504 U.S. at 560–61. To prove an injury in fact sufficient "to raise a First Amendment facial challenge. . . a plaintiff must produce evidence of an intention to engage in a course of conduct arguably affected with a constitutional interest but proscribed by [law.]" *Miss. State Democratic Party v. Barbour*, 529 F.3d 538, 545 (5th Cir. 2008)(internal citations omitted).

The Complaint alleges the U.S. Pastor Council has associational standing. *Complaint*, ¶7. The elements of associational standing are that (a) the association's members would otherwise have standing to sue in their own right; (b) the interests the association seeks to protect are germane to its purpose; and (c) neither the claim asserted nor the relief requested requires the participation of individual members in the lawsuit. *Hunt v. Washington State Apple Advertising Comm'n*, 432 U.S. 333, 343 (1977). When standing is challenged on the basis of the pleadings, a court must accept as true all material allegations of the complaint and construe it in the plaintiff's favor. *Ass'n of Am. Physicians & Surgeons, Inc. v. Texas Med. Bd.*, 627 F.3d 547, 550 (5th Cir. 2010). Nevertheless, the plaintiff claiming associational standing must still plausibly plead those elements, which requires sufficient factual allegations to support an inference of standing. *Draper v. Healey*, 827 F.3d 1, 3 (1st Cir. 2016) (Souter, J., sitting by designation) ("[W]here standing is at issue, heightened specificity is obligatory at the pleading stage. . . the complainant must set forth reasonably definite factual allegations, either direct or inferential, regarding each material element needed to sustain standing.") (internal quotation omitted).

2. Organizations generally do not have associational standing to bring a Free Exercise claim.

Plaintiff cannot establish associational standing to bring a Free Exercise claim because those claims generally cannot be brought by associations and must be brought by the actual parties

claiming deprivation of their rights under the First Amendment. *Harris v. McRae*, 448 U.S. 297 (1980); *Cornerstone Christian Schools v. University Interscholastic League*, 563 F.3d 127, 133–35 (5th Cir. 2009); *Society of Separationists, Inc. v. Herman*, 959 F.2d 1283, 1288–89 (5th Cir. 1992) (en banc), *cert. denied*, 506 U.S. 866 (1992). Free Exercise claims generally require individual participation to establish the alleged coercive effects of the challenged governmental action and, accordingly, are unsuited for an organization to bring on behalf of alleged members. *Harris*, 448 U.S. at 320–21; *Cornerstone Christian Schools*, 563 F.3d at 133–34; *Society of Separationists*, 959 F.2d at 1288–89. This is especially true in this case. None of the Austin member churches whose interests are alleged to be at stake in this litigation are even identified. There are no allegations as to what non-clergy positions any of the 25 member churches may seek to fill in Austin, or in what manner hiring homosexuals, transgendered individuals or women for those jobs would restrict those churches from freely exercising their religion. None of the churches are alleged to have been a respondent in a complaint charging a violation or the Ordinance or have been actually confronted with the theoretical dilemma the Complaint describes regarding church hiring decisions. Even when the claim is purely one for prospective relief, this principle regarding associational standing applies. *Cornerstone Christian Schools*, 563 F.3d at 134 n.5. If this general proposition regarding standing were to somehow not apply to Plaintiff's associational claims in this case, Plaintiff has not pleaded any facts indicating that an exception to the general rule applies.

The same applies to Plaintiff's state law claims. The Texas constitutional claim is evaluated under the same legal standards as the First Amendment Free Exercise claim. *See HEB Ministries, Inc. v. Texas Higher Educ. Coordinating Bd.*, 235 S.W.3d 627, 649–50 (Tex. 2007) ("We have treated the state and federal Free Exercise guarantees as coextensive absent parties' argument to the contrary.") Because the same issue is presented, the requirements of associational standing

must be met. Furthermore, Plaintiff's TRFRA claim is footed on the same purported constitutional rights and the authority regarding associational standing must be satisfied. Finally, under TRFRA, a person may not bring suit unless "*the person* gives written notice to the government agency" at least 60 days in advance, specifying how "*the person's* free exercise of religion is substantially burdened . . ." Tex. Civ. Prac. & Rem. Code §110.006(a)(emphasis added). There is no allegation any of the unnamed Austin church members ever provided the City the required pre-suit notice.[10]

3. Plaintiff failed to plausibly plead it is a membership organization.

Plaintiff has not established associational standing because the Complaint did not plausibly plead it is a membership organization authorized to litigate the rights of its purported Austin church members. To have standing, an association must be a membership organization or its functional equivalent. *See Hunt*, 432 U.S. at 343–45. All Plaintiff has alleged is that it is a non-profit corporation comprised of "approximately 1,000 member churches, including 25 in the city of Austin." *Complaint*, ¶3. Plaintiff conclusory claims the 25 Austin churches are its "members" without identifying them or explaining what that term means. Labels and conclusions do not suffice. Plaintiff must plausibly allege it is a membership organization in order to show associational standing such that this court has jurisdiction over its claims. *Compare Hunt*, 432 U.S. at 344–45 (holding apple growers and dealers were functionally members of apple advertising commission because "[t]hey alone elect the members of the Commission; they alone may serve on the Commission; [and] they alone finance its activities, including the costs of this lawsuit, through assessments levied upon them") *with, e.g., Gettman v. DEA*, 290 F.3d 430, 435 (D.C. Cir. 2002)

[10] The pre-suit notice letter the U.S. Pastor Counsel sent to the City simply states it was written on behalf of that organization and "on behalf of all churches . . . that object to these lifestyles and behavior" and advises "[y]our ordinance substantially burdens the religious freedom *of the U.S. Pastor Council . . .*" *Complaint*, Ex. 5. There is no indication any of the 25 unnamed Austin church members endorsed, authorized, approved or participated in the notice letter, or that those members prompted the filing of this lawsuit.

(holding *High Times* magazine lacked associational standing to petition for review of Drug Enforcement Administration decision denying petition to initiate rulemaking proceedings to reschedule marijuana where magazine's readers and subscribers did not select leadership, guide activities, or finance activities of the magazine and dismissing petition).

4. Plaintiff failed to plausibly plead that its 25 Austin church members have individual standing.

The Complaint failed to establish associational standing because it does not identify any alleged members of the U.S Pastor Council who are subject to the Ordinance. *Summers v. Earth Island Inst*., 555 U.S. 488, 498–99 (2009) (holding that an association claiming standing must "identify [a] member[] who ha[s] suffered the requisite harm.") (emphasis added). Furthermore, the Complaint merely alleges Plaintiff has 25 members churches in Austin, without alleging any of those churches are covered employers under the Ordinance. The Ordinance defines "employer" as a person who has 15 or more employees for each working day in each of the 20 or more calendar weeks in the current or preceding calendar year (with certain exceptions, chiefly government employers). *Austin City Code* §5-3-1(10). The Complaint does not allege any of the 25 Austin member churches have the requisite number of employees to be an "employer" subject to the Ordinance. If the unnamed churches are not employers covered by the Ordinance, then they have no standing to bring this suit. Plaintiff's conclusory assertion that "its Austin member churches would have standing to sue in their own right," does not satisfy pleading requirements. *Complaint*, ¶7. Labels and conclusions, formulaic recitations of elements, and naked assertions devoid of further factual enhancement are insufficient to meet the plausibility standard. *See Ashcroft v. Iqbal*, 556 U.S. 662, 678 (2009).

Even if the 25 member churches did have the requisite number of employees, the Complaint

does not allege they have suffered, or face any impending injury. The first required element of standing is "an injury in fact, which is concrete and particularized invasion of a legally protected interest." *Nat'l Rifle Ass'n v. BATFE*, 700 F.3d 185, 190-91 (5th Cir. 2012). There is no allegation that any of the 25 Austin church members have been confronted with the choice of hiring, or not hiring, an objectionable female, homosexual or transgendered individual for any job in Austin. Nor is there any claim that such a choice is imminent, or even likely to occur. The Supreme Court has "repeatedly reiterated that threatened injury must be *certainly impending* to constitute injury in fact and that allegations of possible future injury are not sufficient." *Clapper v. Amnesty Int'l USA*, 568 U.S. 398, 409 (2013)(emphases in original; internal citation omitted). These requirements assure that there is "a real need to exercise the power of judicial review in order to protect the interests of the complaining party." *Summers v. Earth Island Inst*., 555 U.S. 488, 493 (2009). The Complaint alleges no impending injury faced by the 25 Austin church members.

5. Plaintiff failed to plausibly plead that the litigation is germane to its purpose.

Plaintiff did not establish associational standing because the Complaint failed to plausibly plead that the interests it seeks to protect through this litigation are germane to its organizational purpose. *Hunt*, 432 U.S. at 343. "[T]he germaneness requirement is 'undemanding' and requires 'mere pertinence' between the litigation at issue and the organization's purpose. *Ass'n of Am. Physicians & Surgeons, Inc. v. Texas Med. Bd.*, 627 F.3d 547, 550 n.2 (5th Cir. 2010) (*quoting Bldg. & Constr. Trades Council of Buffalo v. Downtown Dev., Inc*., 448 F.3d 138, 148 (2nd Cir.2006)). Nevertheless, it still requires a plaintiff claiming associational standing to actually plead a purpose which has, at least, "mere pertinence." *See id*.

The Complaint is devoid of any allegation as to the purpose of the U.S. Pastor Council. The Complaint conclusorily states "the rights of religious freedom and church autonomy that [Plaintiff]

seeks to vindicate in this lawsuit are germane to the organization's purpose", but does not allege what that purpose is. Complaint ¶ 7. This omission is critical and requires dismissal of the claim. *See McKinney v. U.S. Dep't of Treasury*, 799 F.2d 1544, 1553 (Fed. Cir. 1986) (upholding dismissal of complaint for lack of standing where all alleged organization pleaded to show associational standing was that it was a 'nonprofit public interest law firm,' without indicating an alleged purpose of that firm which was germane to the litigation); *cf. Individuals for Responsible Gov't, Inc. v. Washoe Cty. By & Through the Bd. of Cty. Comm'rs*, 110 F.3d 699, 702 (9th Cir. 1997), *cert. denied*, 522 U.S. 966 (1997) (affirming summary judgment that nonprofit plaintiff lacked associational standing where plaintiff failed to specify its members and the organization's purpose; "[a]bsent both purpose and members, it lacks any standing to sue").

B. The claims are not ripe

1. The standard of review regarding ripeness.

The ripeness doctrine "is drawn both from Article III limitations on judicial power and from prudential reasons for refusing to exercise jurisdiction." *Reno v. Catholic Soc. Servs., Inc*., 509 U.S. 43, 58 n. 18 (1993). Its "basic rationale is to prevent the courts, through avoidance of premature adjudication, from entangling themselves in abstract disagreements." *Abbott Labs. v. Gardner*, 387 U.S. 136, 148 (1967). Determining whether a claim is ripe for judicial review requires the evaluation of (1) "the fitness of the issues for judicial decision" and (2) "the hardship to the parties of withholding court consideration." *Abbott Labs*., 387 U.S. at 149. If the controversy is "abstract or hypothetical," the declaratory judgment action is premature. *United Transp. Union v. Foster*, 205 F.3d 851, 857 (5th Cir. 2000). Conversely, an actual controversy is present where "a substantial controversy of sufficient immediacy and reality [exists] between parties having adverse legal interests." *Middle South Energy, Inc. v. City of New Orleans*, 800 F.2d

488, 490 (5th Cir. 1986). Even if the "actual controversy" requirement is satisfied, however, a district court retains broad discretion to decide or dismiss a declaratory judgment action. *See Travelers Ins. Co. v. Louisiana Farm Bureau Fed'n, Inc*., 996 F.2d 774, 778 (5th Cir. 1993). Applying the ripeness doctrine in the declaratory judgment context presents "a unique challenge." *Orix Credit All., Inc. v. Wolfe*, 212 F.3d 891, 896–97 (5th Cir. 2000). This is so because "declaratory judgments are typically sought before a completed 'injury-in-fact' has occurred. . . but still must be limited to the resolution of an 'actual controversy.'" *Foster*, 205 F.3d at 857 (5th Cir. 2000); see also 28 U.S.C. § 2201(a).

2. The Complaint does not allege any of the 25 Austin member churches are covered by the Ordinance.

As noted above, Plaintiff's claims are not ripe because the Complaint fails to plausibly plead the alleged Austin member churches are Austin "employers" covered by the Ordinance. *See Int'l Tape Mfrs. Ass'n v. Gerstein*, 494 F.2d 25, 28–29 (5th Cir. 1974) (no justiciable controversy where complaint failed to allege that association's member "are now, or ever will be," subject to the Florida law they challenged). Absent such allegation, Plaintiff's challenges to the Ordinance are not ripe.

3. The Complaint does not allege any threatened enforcement action.

There is no allegation that any of Plaintiff's Austin church members -- or any church, for that matter -- have been the subject of an enforcement action under the Ordinance for failing to hire female, homosexual and/or transgendered individuals for any job. There is also no allegation that any such complaint under the Ordinance has ever been made, or that the complaint was deemed sufficient by the City, that an EEFH investigator found reasonable cause to believe the charge, and that efforts at resolving the complaint had failed, all prerequisites to the potential of prosecution. *Austin City Code*, §5-3-7, 8 and 12. There is no allegation that a homosexual or transgendered

person ever applied for a job and was turned down for religious reasons at any of the 25 Austin churches. In short, no enforcement action, or even an event that might lead to a potential enforcement action, is alleged in the Complaint. Such events remain entirely conjectural and hypothetical, and are not fit issues for judicial determination.

4. The Complaint does not plausibly allege any Austin member church intends to engage in activity proscribed by the Ordinance.

Although in some cases a plaintiff may make a pre-enforcement challenge to a law, the Complaint alleges no such justiciable controversy such that this Court may adjudicate the claims as a pre-enforcement challenge. For such a claim to be justiciable, the plaintiff must allege both an "intention to engage in a course of conduct arguably affected with a constitutional interest, but proscribed by statute" and "a credible threat of prosecution thereunder." *Speaks v. Kruse*, 445 F.3d 396, 399 (5th Cir. 2006) (*quoting Babbitt v. United Farm Workers Nat'l Union*, 442 U.S. 289 (1979)). To meet the first element, a plaintiff must allege a "'serious [] interest in acting contrary to a [law.]" *Nat'l Fed'n of the Blind of Texas, Inc. v. Abbott*, 647 F.3d 202, 209 (5th Cir. 2011) (*quoting Miss. State Democratic Party v. Barbour*, 529 F.3d 538, 545 n.8 (5th Cir. 2008)). The second element—the credibility of a threat of prosecution—is determined by assessing "the practical likelihood that a controversy will become real." *Shields v. Norton*, 289 F.3d 832, 835 (5th Cir. 2002). This requirement recognizes the critical distinction between actual and hypothetical controversies.

Hoyt v. City of El Paso, Tex., illustrates the limits of a court's jurisdiction over such pre-enforcement claims. 878 F. Supp. 2d 721 (W.D. Tex. 2012) In *Hoyt*, a pastor and his church brought claims under the U.S. Constitution's Due Process, Equal Protection, and Free Exercise clauses to challenge a provision in the Texas Elections Code which they claimed caused them to refrain from circulating recall petitions out of fear of civil and criminal penalties. *Id.* at 723. But

the pastor and his church had not shown that the activity they sought to engage in—namely, circulating recall petitions—would violate the challenged Election Code provision, which prohibited corporate and labor union contributions in connection with a recall election. *Id*. at 736. Nor had they alleged what they sought to contribute, how they wanted to contribute, or to whom they wanted to contribute. *Id*. The pastor plaintiff failed to allege that the challenged provision even operated against him, since it only applied to corporations and labor organizations and he had not alleged any facts showing it was applicable to him personally. Both the pastor and the church had further failed to show a credible threat of prosecution. *Id.* at 737-38. While they had alleged the existence of a lawsuit and grand jury proceedings concerning violations of the Election Code, there was no allegation about those proceedings which showed that they involved conduct similar to the conduct the pastor and church wished to engage in. These multiple failings resulted in dismissal for lack of a justiciable controversy because the court refused to render "an advisory opinion on an abstract legal issue." *Id*. at 739.

Here, there is even less of a potential justiciable controversy than in *Hoyt*. Plaintiff has not plausibly alleged that the Ordinance applies to it or to its 25 Austin church members. As discussed below, there is no plausible allegation that certain conduct Plaintiff claims the Ordinance prohibits—specifically with regard to hiring certain individuals as clergy —is actually prohibited. And while in *Hoyt*, the plaintiffs alleged they were refraining from engaging in conduct they believed was prohibited by law, there is no such comparable allegation in Plaintiff's Complaint. While Plaintiff conclusorily alleges any "law that purports to regulate church hiring decisions inflicts injury in fact by restricting the church's autonomy," there is no allegation that any Austin church member's autonomy has been restricted in any way.

In other words, there is no allegation that Plaintiff's alleged Austin church members have suffered, or would even be likely to suffer, any harm in the absence of the adjudication of these claims. *Abbott Labs.*, 387 U.S. at 152–53 (to constitute hardship, impact of law must be "sufficiently direct," resulting in an "immediate and significant change in the plaintiffs' conduct"). No jurisdiction exists for this court to decide Plaintiff's hypothetical and speculative claims for relief. For this Court to dismiss these unripe claims poses no hardship to the parties.

C. Plaintiff failed to state a claim

1. Standard of review for failure to state a claim.

Under the procedural rules, a complaint must contain "a short and plain statement of the claim showing that the pleader is entitled to relief" and a demand for the relief sought. Fed.R.Civ.P. 8(a)(2) & (3). Each factual allegation in the pleading must be simple, precise and direct. Fed.R.Civ.P. 8(d)(1). To survive a motion to dismiss for failure to state a claim under Rule 12(b)(6), a plaintiff must allege sufficient facts to state a plausible claim. A complaint must contain sufficient factual matters, accepted as true, to state a claim to relief that is plausible on its face. *Bell Atl. Corp. v. Twombly*, 550 U.S. 544, 570, 127 S.Ct. 1955 (2007). The plausibility requirement means that a plaintiff must plead a comprehensible legal theory and must allege facts, not conclusions, establishing every essential element of the legal theory on which the claim for relief is based. *Ashcroft*, 556 U.S. at 678. On a motion to dismiss, the court must accept the plaintiff's factual allegations as true, drawing all reasonable inferences in plaintiff's favor. However, conclusory allegations or legal conclusions are not entitled to the assumption of truth, and a court may disregard them. *Id.* This determination depends upon the context and requires the court to draw upon its own experience and common sense. *Id.* The court's review is limited to the complaint, any documents attached to the complaint, and any documents attached to the

motion to dismiss that are central to the claim and referenced by the complaint. *Lone Star Fund V (U.S.), L.P. v. Barclays Bank PLC*, 594 F.3d 383, 387 (5th Cir. 2010).

2. No facts alleged to show the Ordinance applies to Plaintiff or its 25 Austin church members.

The U.S. Pastor Council is not based in Austin but is headquartered in Houston. *Complaint*, ¶3. The Complaint does not allege the organization has an office or any employees in Austin. There is no allegation the organization is a church. Although the Complaint purports to bring the claims on behalf of parties other than the U.S. Pastor Council, even if associational standing was not invoked, the Complaint does not allege facts supporting a comprehensible legal theory that the Ordinance impacts Plaintiff's employment and hiring practices.

As noted, there are no facts alleged to establish that any of Plaintiff's 25 Austin church members have the sufficient number of Austin employees to be considered "employers" subject to the Ordinance. Therefore, the Complaint does not allege facts from which the Court could plausibly infer that Plaintiff or any of the 25 unnamed Austin church members are subject to the Ordinance, or that their constitutional rights to freely exercise their religion are impinged.

3. Clergy-hiring decisions by churches are exempt from the Ordinance.

Even if the 25 Austin member churches had the requisite number of Austin employees to be considered "employers" under the Ordinance, it would not prohibit those churches from refusing to hire women, practicing homosexuals or transgendered people as clergy. That is because of the "ministerial exception" recognized by state and federal courts. *See Hosanna-Tabor Evangelical Lutheran Church and School v. E.E.O.C.*, 565 U.S. 171, 132 S.Ct. 694 (2012); *Patton v. Jones*, 212 S.W.3d 541, 547 (Tex.App. – Austin 2006). In *Hosanna-Tabor*, an ADA case, the Supreme Court held the First Amendment's Establishment and Free Exercise clauses bar suits brought on behalf of ministers against their churches where statutorily-prohibited employment

discrimination is alleged. 565 U.S. at 188-90. The so-called ministerial exception exempts clergy hiring decisions by churches from employment discrimination statutes, including the Ordinance.

The ministerial exception has been applied to federal employment discrimination laws in Texas since before the Ordinance was enacted. *See McClure v. Salvation Army*, 460 F.2d 553, 557 (5th Cir. 1972)(Ministerial exception exempts employment relationship between churches and their ministers from Title VII). Section 5-3-3 of the Ordinance, entitled "Interpretation and Designation" directs that:

> In construing this chapter, it is the intent of the city council that the courts shall be guided by the rules and regulations of the EEOC and the Federal Court interpretations of Title VII of the Civil Rights Act of 1964, the Americans with Disabilities Act, the Age Discrimination in Employment Act of 1967, and Chapter 21 (Employment Discrimination) of the Texas Labor Code.

This intent in the Ordinance is comparable to that set out in Texas Labor Code §21.001. *Schroder v. Texas Iron Works, Inc.*, 813 S.W.2d 483, 485 (Tex. 1991)("The expressed purpose of the Texas Commission on Human Rights Act is the execution of the policies embodied in Title VII.")

The Complaint alleges there are "no exceptions" in the Ordinance to exempt clergy hiring decisions of churches. *Complaint*, ¶15. That conclusory allegation is incorrect. The ministerial exception, which is discussed in the *Hosanna-Tabor* case and related authority interpreting federal employment discrimination statutes, guides the interpretation of the Ordinance consistent with the City Council's expressed intent. Although the ministerial exception is not explicitly spelled out in the text of the Ordinance, that is not required. The ministerial exception does not appear in the text of Title VII, Chapter 21 of the Texas Labor Code, the ADA or the ADEA, either. But it is the law.[11]

[11] Plaintiff was well aware of this when it sued the City. In a federal court complaint the U.S. Pastor Council filed last October against the federal government, it alleged "This 'ministerial exception' protects the Catholic church from

The Complaint does not allege the City has ever interpreted or enforced the Ordinance in a manner that is inconsistent with the ministerial exception. Owing to the ministerial exception, the Ordinance would not be enforced against the 25 Austin church members if they refused to hire an individual for a clergy position based on the individual's sex, sexual orientation or gender identity, even if such a complaint were made to the EEFH Office. The theoretical dispute outlined in the Complaint is simply not an issue, and the claim should be dismissed.

4. The Complaint fails to allege a cause of action with regard to a church's non-clergy positions.

i. The law regarding free exercise of religion claims.

To the extent the Complaint alleges a theory that the Ordinance violates the rights of the 25 Austin church members to freely exercise their religion with regard to the hiring of *non-clergy* employees, that claims should be dismissed. Claims of facial invalidity often rest on speculation. As a consequence, they raise the risk of "premature interpretation of statutes on the basis of factually barebones records". *Sabri v. United States*, 541 U.S. 600, 609 (2004) (internal quotation marks and brackets omitted). Facial challenges also run contrary to the fundamental principle of judicial restraint that courts should neither "anticipate a question of constitutional law in advance of the necessity of deciding it" nor "formulate a rule of constitutional law broader than is required by the precise facts to which it is to be applied." *Ashwander v. Tennessee Valley Authority*, 297 U.S. 288, 347 (1936) (Brandeis, J., concurring). Facial challenges threaten to short circuit the democratic process by preventing laws embodying the will of the people from being implemented in a manner consistent with the Constitution, and courts "must keep in mind that "[a] ruling of

being forced to hire women as priests – notwithstanding the text of Title VII – and it protects other churches from being forced to hire practicing homosexuals as clergy." *See* Plaintiffs' Class-Action Complaint, ¶15, filed in *U.S. Pastor Council, et al v. Equal Employment Opportunity Commission, et al*, Cause No. 4:18-cv-824 in the U.S. District Court for the Northern District of Texas, Fort Worth Division.

unconstitutionality frustrates the intent of the elected representatives of the people." *Ayotte v. Planned Parenthood of Northern New Eng.*, 546 U.S. 320, 329 (2006). These imperatives are critical in this case, where the circumstances presenting a conflict between a law and a religious belief have not manifested themselves in a real-world dispute.

The First Amendment's Free Exercise Clause, applicable to the states through the Fourteenth Amendment, prohibits state and local governments from making any "law respecting an establishment of religion, or prohibiting the free exercise thereof. U.S. Const. amends I, XIV; *Cantwell v. Connecticut*, 310 U.S. 296, 303 (1940). Similarly, the Texas Constitution's Bill of Rights, article 1, section 6, entitled "Freedom of Worship", provides that all men have the "right to worship Almighty God according to the dictates of their own consciences" and prohibits a human authority from controlling or interfering with the rights of one's conscience in religious matters. Tex. Const. Art. 1, §6.

Laws that burden the free exercise of religion, as opposed to neutral, generally-applicable laws incidentally burdening religious exercise, must be narrowly tailored to advance a compelling government interest. *Church of the Lukumi Babalu Aye, Inc. v. City of Hialeah*, 508 U.S. 520, 531-32 (1993). A law that is neutral and of general applicability need not be justified by a compelling governmental interest even if the law has the incidental effect of burdening a particular religious practice. *Id., citing Employment Div., Dept. of Human Resources of Ore. v. Smith*, 494 U.S. 872, 110 S.Ct. 1595 (1990). A law lacks facial neutrality if it refers to a religious practice without a secular meaning discernible from the language or context. *See City of Hialeah*, 508 U.S. at 533. Thus in analyzing a plaintiff's free exercise claim, the court must first determine the neutrality and applicability of an ordinance before the alleged restriction on the free exercise of religion can be examined.

ii. Applying the law to the Ordinance.

The text of the Ordinance is facially neutral with regard to religion. *See City of Hialeah*, 508 U.S. at 533 ("[T]he minimum requirement of neutrality is that a law not discriminate on its face."). A law is not facially neutral if it "refers to a religious practice without a secular meaning discernable from the language or context." *Id*. On its face, the Ordinance does not establish a religion or prohibit any Austinite from freely practicing their chosen religion. The Complaint points to no "religious practices" referenced in the Ordinance, and no religious practice is alleged to be targeted, either overtly or covertly, by the Ordinance. There is no indication, or allegation, that the objective of the Ordinance's restrictions regarding discriminatory employment practices based on an individual's sex, sexual orientation or gender identity is to infringe upon the free exercise of an employer's religion. Therefore, the Ordinance is facially neutral.

The Ordinance is generally applicable, with exemptions noted in the text and common law.[12] The City Code section containing the Ordinance states at the outset that it is aimed at prohibiting certain discriminatory hiring and employment practices in the City of Austin. *Austin City Code* §5-3-1. The policy the Ordinance was intended to further is based on "the recognition of the inalienable rights of each individual to work to earn wages and obtain a share of the wealth of this City through gainful employment" and that ". . . the denial of such rights [through prohibited discrimination] is detrimental to the health, safety and welfare of the inhabitants of the City." *Id*., §5-3-1(B). The policy imperatives articulated in the Ordinance, namely the recognition that *all* Austinites have the right to make a living through gainful employment, are of legitimate concern

[12] For example, the Ordinance exempts situations where an individual's religion, sex, sexual orientation, gender identity, national origin, age or disability are a "bona fide occupational qualification reasonably necessary for the employer's business or enterprise." The Ordinance also permits religious corporations and associations to hire and employ individuals of a particular religion to perform work connected with their activities. *Austin City Code*, §5-3-15(A) & (C).

to a municipality. Those policies apply to all individuals and not according to their religious beliefs. With regard to the church's hiring of non-clergy employees, the Ordinance does not burden any religious practice identified in the Complaint.

On its face, the challenged Ordinance is a general law that applies to all non-exempt employers in Austin, as that term is defined. Even if that were not the case, no employers are targeted based on their religious beliefs, and there are contrary allegations in the Complaint. The Ordinance is aimed at any discriminatory acts, as listed, by non-exempt Austin employers, whether the act is motivated by religious beliefs or something else.

D. The Court lacks jurisdiction over Plaintiff's TRFRA claim for failure to satisfy the pre-suit notice requirement.

Under the TRFRA, the mandatory, pre-suit written notice to the government agency sued must state the following: (1) that the person's free exercise of religion is substantially burdened by an exercise of the government agency's authority; (2) a description of "the particular act or refusal to act that is burdened"; and (3) "the manner in which the exercise of governmental authority" burdens the person's act or refusal to act. Tex.Civ.Prac. & Rem. Code §110.006(a)(1-3). These pre-suit notice requirements are jurisdictional and are strictly construed. *See Morgan v. Plano Indep. School Dist.*, 724 F.3d 579, 588 (5th Cir. 2013)(Section 110.006 of the TRFA requires timely pre-suit notice in the form of certified mail, return receipt requested in order to waive a government agency's immunity.) As the court noted in *Morgan*, under state law "statutory prerequisites to a suit, including the provision of notice, are jurisdictional requirements against a governmental entity," and when they are not met the government's immunity from suit is not waived. *Id.*, *citing* Tex. Govt. Code §311.034. This requirement serves the salutary purpose of notifying the government exactly what on is complaining about before subjecting them to jurisdiction of liability.

Plaintiff's pre-suit notice letter to the City did not satisfy the TRFRA's notice requirements regarding Plaintiff's claims as to hiring *non-clergy* employees. In the letter, the president of the U.S. Pastor Council notified Austin's Mayor that the Ordinance "violates state law because it fails to protect the autonomy and religious freedom of churches . . . that hold a sincere religious objection to homosexuality, transgender behavior, and women *serving as clergy*." *Complaint*, Ex. 5. The letter refers multiple times to churches who object to hiring certain individuals "as clergy", but there is no expressed objection that the Ordinance imposes a burden on the religious freedom of churches when hiring *non*-clergy employees. Because the pre-suit letter notified the City only of a complaint regarding a church's hiring of clergy, the Plaintiff did not provide the requisite pre-suit notice of its claim regarding the refusal to hire *non-clergy* employees.[13] The City has not waived its governmental immunity to suit regarding that TRFRA claim and the Court lacks subject matter jurisdiction to hear it. The claim should be dismissed under Fed.R.Civ.P. 12(b)(1). Alternatively, the Court should not exercise its supplemental jurisdiction to hear this state law claim.

E. The Complaint against Mayor Steve Adler in his official capacity fails to state a claim.

Suits to redress deprivations of federal civil rights by persons acting "under the color of any [state] statute, ordinance, regulation, custom or usage" are authorized under 42 U.S.C. §1983. The Complaint does not allege any facts establishing that Adler enforces the challenged Ordinance, and more specifically why his presence as a party is necessary for the Court to grant the declaratory and injunctive relief Plaintiff seeks. In fact, the opposite is true.

[13] As noted, under the ministerial exception the Ordinance does not apply to qualifying Austin churches in connection with hiring clergy. Therefore, the Ordinance does not "substantially burden" the free exercise of religion by any of the 25 Austin member churches when they hire employees for clergy positions. Because the Complaint fails to allege a TRFRA claim on that theory, the claim should be dismissed.

The City of Austin has a council-manager form of government. The City's day-to-day operations are run by the City Manager, not the Mayor, who is the ceremonial head of government only. Austin's Mayor does not have executive authority over City operations. See Charter of the City of Austin, art 1, §2 (vesting executive functions in the City Manager). See Ex. 3. As Mayor, Defendant Adler does not oversee or direct enforcement of the Ordinance Plaintiff challenges and the Complaint alleges no such facts. Because Mayor Steve Adler is not a City executive who can be compelled to perform an administrative action in connection with the Ordinance, Plaintiff has failed to state a cause of action against him and he should be dismissed.

PRAYER

Defendants pray that the Court grant its motion to dismiss, and that an order be entered dismissing all claims against Defendants in this lawsuit with prejudice, or that those claims discussed individually above be dismissed, and grant Defendants such further relief as the Court deems just and proper.

RESPECTFULLY SUBMITTED,

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EXHIBIT P

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THE REGRESSION FALLACY

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This paper examines mathematically the origin of the regression toward the mean phenomenon. The magnitude of regression effect in terms of mean and mean percentage change for a selected target population was calculated under the assumption of a bivariate normal distribution. The impact of regression effect on the statistical estimation of a treatment effect was addressed. Methods to reduce and to adjust for regression effect were reviewed and discussed. Questions concerning the designs and the analyses of clinical trials in light of the presence of regression effect were posed.

Key Words: Baseline; Correlation coefficient; Mean change; Mean percentage change; Reliability measure; Significance test

INTRODUCTION

STATISTICAL REGRESSION describes a tendency of extreme measurements to move closer to the mean when they are repeated (1). This phenomenon was first observed by Galton (2) when experimenting with the growth of peas. Galton observed that the crossing of two tall plants produced offspring which were, on average, shorter than either parent plant. Likewise, the crossing of two short plants produced offspring which were generally taller than either parent (3). Galton also observed this phenomenon in humans.

Regression effect also explains why in a test-retest situation, the bottom group on the first test will on average show some improvement on the second test while the top group will on average fall back (4). Another example of regression effect is the observation that upon administration of a serum cholesterol lowering diet, individuals with initially high serum cholesterol concentrations generally experience greater reductions than individuals with initially lower concentrations (5). The amount of improvement due to regression can be large and statistically significant as will be demonstrated. Given observations (X, Y), regression toward the mean occurs whenever X and Y are positively correlated and have identical marginal distributions (1), even though it can happen under less stringent conditions.

This paper first examines the source of regression effect, followed by a demonstration of the magnitude of this effect for both the absolute and the percentage change, as well as a discussion on how regression effect can distort the interpretation of significance test results. The amount of change due to regression is related to the distance between the pretreatment measurement and the population mean. It is also related to the reliability measure between a measurement and a remeasurement of the same parameter. Methods to reduce and adjust for regression effect are reviewed and discussed. The appropriateness of some common statistical practice in view of the results noted will be discussed, and questions for further consideration will be posed.

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SOURCE OF REGRESSION EFFECT

This section provides a mathematical description of the source for the regression toward the mean phenomenon. To facilitate the discussion, the authors assume that X and Y represent repeated measurements on the same subject with X preceding Y, and that in the absence of any intervention or any intervention effect, the parameter which X and Y measure is subject only to random biological fluctuation. To describe and quantify regression effect, it is assumed that X and Y have the same univariate normal distribution $N(\mu,\sigma^2)$ in the absence of any treatment intervention. It is also assumed that X and Y jointly have a bivariate normal distribution with a positive correlation coefficient ρ, and therefore a positive covariance $\rho\sigma^2$.

Under the bivariate normal distribution assumption, the conditional distribution of Y given $X = x$ is also normal with a mean of $\mu + \rho(x - \mu)$ and a variance given by $\sigma^2(1 - \rho^2)$. Notice that the conditional mean is a linear function of x. Therefore, if one plots the conditional mean $E(Y|X = x)$ against x as in Figure 1, one will get a line which goes through (μ,μ) (mean of the bivariate normal distribution). Because the positive correlation coefficient ρ is typically less than one for biological measurements, the slope of this line is less than one. This result implies that for a given x greater than μ, the conditional expectation of Y given x is smaller than x itself. This mathematical relationship explains the test/retest phenomenon mentioned earlier. A corollary of this is that the average of the conditional expectation over a region R defined by $R = \{x|x \geq a\}$ with $a > \mu$ is smaller than the average of X over R, that is, the inequality in (2.1).

$$E(Y|X \geq a) \leq E(X|X \geq a) \quad (2.1)$$

For convenience, it is assumed that high values of X and Y imply certain biological or biochemical abnormality and are therefore undesirable. Thus, if a treatment is given, one of the objectives is to see if the treatment can bring the measurement down. Under such a scenario, a negative mean difference ($E(Y - X)$) that has a *large* absolute value is often considered a sign for a treatment effect. In the absence of any intervention, one can still consider the conditional mean change (conditional on the value of x). Such a conditional mean change under the bivariate normal distribution assumption is

$$E(Y - X|X = x) = (x - \mu)(\rho - 1) \quad (2.2)$$

The conditional mean change in (2.2) consists of two multiplicative terms. The first term gives the distance between x and its mean value while the second is related to the index of reliability. The result in (2.2) suggests that the absolute conditional mean change increases as x becomes more undesirable and/or as the reliability of measurement/remeasurement decreases. In other words, on average, greater changes can be expected if the first measurement displays a higher degree of abnormality. Furthermore, more changes can be expected if the measurement/remeasurement displays a high degree of inconsistency or a low degree of reproducibility. One can apply this finding to situations where X represents a pretreatment measurement and Y a posttreatment measurement when the treatment administered has no effect on the parameter. In the latter case, the amount of the conditional mean change is proportional to the degree of baseline abnormality and $(\rho - 1)$.

Using (2.2), the result in (2.3) which gives the conditional mean percentage change in the absence of any treatment effect is easily obtained.

$$E\left(\frac{Y - X}{X} \mid X = x\right) = (\rho - 1)\left(1 - \frac{\mu}{x}\right) \quad (2.3)$$

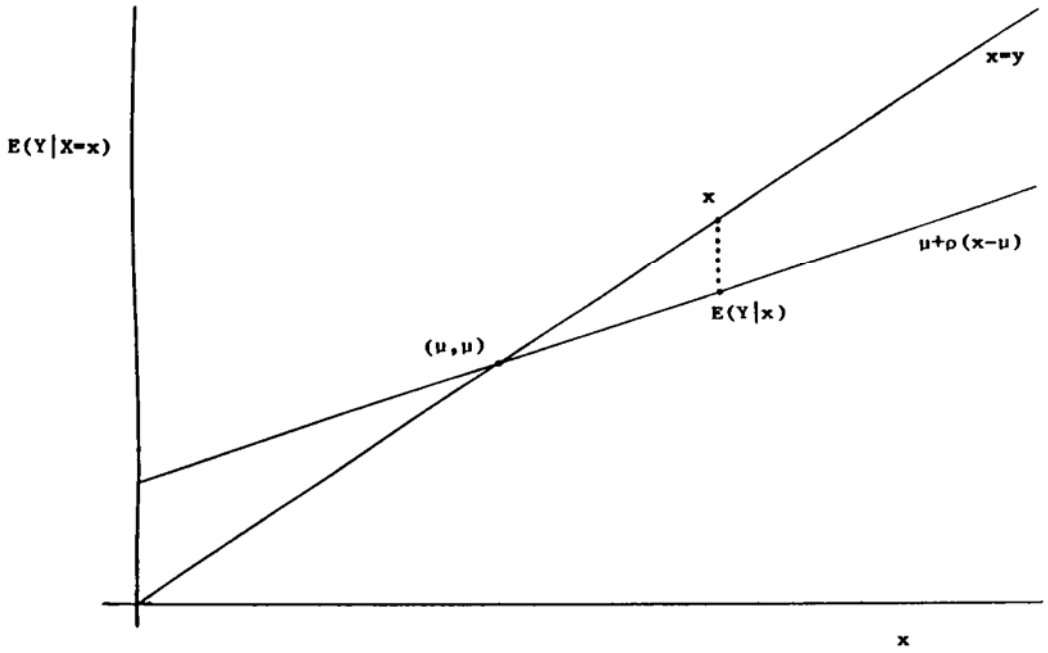

FIGURE 1. Plot of $E(Y \mid X = x)$ against x.

MAGNITUDE OF REGRESSION EFFECT

Since regression toward the mean affects the conditional mean (and mean percentage) changes at the two ends of the baseline measurement in an opposite manner, its average effect over all possible values of X is zero. Unfortunately, most clinical trials are targeted toward patients with a certain degree of abnormality, say, reflected by X. In other words, the treatment under investigation is frequently only offered to patients with high X values. Assume for the moment that the study has an inclusion criterion based on one single pretreatment measurement x being at least two standard deviations above the population mean μ. This translates to x being at least $\mu + 2\sigma$. From the result in (2.2), it can be seen that the absolute conditional mean change due to regression alone is at least $2\sigma(1 - \rho)$ for $x \geq \mu + 2\sigma$. Therefore, the absolute value of the mean change obtained by averaging the conditional mean change over the region defined by $\{x | x \geq \mu + 2\sigma\}$ is at least $2\sigma(1 - \rho)$. The actual mean change due to regression alone is

$$\begin{aligned} E_{X \geq \mu + 2\sigma} E(Y - X \mid X) &= \frac{(\rho - 1)\sigma}{0.0228} \int_2^\infty z\phi(z)dz \\ &= \frac{(\rho - 1)\sigma}{0.0228} \frac{e^{-2}}{\sqrt{2\pi}} \\ &= 2.368(\rho - 1)\sigma \qquad (3.1) \end{aligned}$$

where $\phi(\cdot)$ is the density function of the standard normal distribution. The quantity 0.0228 in the denominator is the right tail probability of the standard normal distribution beyond two. In general, if the study selects the top $(100f)$ percentage of the X-distribution and if T is the upper

$(100f)^{th}$ percentile of the standard normal distribution (ie, $T = \Phi^{-1}(1 - f)$ where $\Phi(\cdot)$ is the cumulative distribution of the standard normal distribution), then the mean change due to regression based on one single pretreatment measurement is given by

$$E_{X \geq \mu + T\sigma} E\,(Y - X \mid X) = \frac{(\rho - 1)\sigma}{f} \frac{e^{-\frac{T^2}{2}}}{\sqrt{2\pi}} \qquad (3.2)$$

With an estimate for the population standard deviation σ and a good feeling for the measurement/remeasurement correlation coefficient, the mean change due to regression can be estimated using (3.2).

How does regression effect affect obtaining unbiased statistical inference? How often is a significant mean change declared when what is observed is actually regression effect and not a true treatment effect? Consider the case where patients are selected based on one single pretreatment measurement being at least two standard deviations above the mean. It can easily be shown that the (unconditional) variance of $(X - Y)$ is $2(1 - \rho)\sigma^2$. Using this variance, the sample size necessary to have a 95% power to declare a significant mean change that is due to regression at the 5% significance level can be computed. Such sample sizes for ρ between 0.4 and 0.9 are given in Table 1. The required sample sizes in Table 1 vary from 8–47. The results in Table 1 imply that if the inclusion criterion selects the top 2.28% of the X-distribution, the almost certain conclusion is that the mean change is significantly different from zero in a single-arm Phase II trial with a sample size between 20 and 30. The false positive rate (ie, the chance of declaring a treatment effect that does not exist) is almost 100% when the reliability of measurement/remeasurement is low. A question arises naturally: Is an effort routinely made to account for regression effect or do researchers happily conclude on most occasions that the treatment under investigation works? The author's personal experience is that while most researchers are aware of the existence of regression effect, they don't conscientiously try to adjust the observed mean difference for the mean change expected from such effect. As a result, the effect of a treatment for a target patient population may be greatly exaggerated. This can be detrimental to a drug development program if efficacy is erroneously concluded from Phase II single-arm efficacy trials when the concluded efficacy is nothing but regression effect. If Phase III randomized trials are launched based on these false conclusions, resources will be wasted and a disservice to society will be committed.

TABLE 1
Sample Sizes Required to Have a 95% Power and a 5% Significance Level to Detect a Mean Change of 2.368 $(\rho - 1)\,\sigma$ for Various ρ Values Using a Two-sided Test

Correlation Coefficient ρ	Sample Size Required
0.4	8
0.5	10
0.6	12
0.7	16
0.8	24
0.9	47

How do things look for the percentage change? Is percentage change exempt from regression effect? One might think that since the change is divided by the corresponding pretreatment measurement to obtain the percentage change, a large absolute change because of a high pretreatment value does not necessarily lead to a high percentage reduction. Assuming again that the upper $(100f)$ percent of the X-distribution is selected, the mean percent change due to regression under the bivariate normal distribution assumption can be obtained by averaging the conditional mean percentage change in (2.3)

over the interval of $(\mu + T\sigma, \infty)$ where T is the upper $(100f)^{th}$ percentile of the standard normal distribution. This mean percentage change can be equivalently expressed as

$$E\left(\frac{Y - X}{X}\right) = (\rho - 1) \times \left\{1 - \frac{1}{f}\int_T^\infty \left(\frac{1}{1 + \frac{\sigma}{\mu}z}\right)\phi(z)dz\right\} \quad (3.3)$$

where $\phi(\cdot)$, as before, represents the density function of the standard normal distribution.

McDonald, Mazzuca, and McCabe (1) gave some examples of the size of the absolute mean percentage change (or equivalently, the mean percentage reduction) due to regression when the selection criterion based on one single pretreatment measurement requires the measurement to be at least three standard deviations above the mean. Using the means, standard deviations, and the measurement/remeasurement correlation coefficient estimates for 15 biochemical assays obtained by Cotlove, Harris, and Williams (6) from some long-term studies in normal subjects, McDonald, Mazzuca, and McCabe computed the mean percentage change for this target patient population. The mean percentage reduction due to regression was found to range between 2.5% (sodium) and 26% (LDH)! These findings are particularly disturbing considering that most regimens aim to achieve an average percentage reduction between 10% and 30%! In other words, a sample size that is adequate to pick up a 10%–30% treatment effect is often large enough to pick up a mean percentage change that is entirely due to regression!

REMEDIES FOR REGRESSION EFFECT

In view of these results, what can be done to help reduce regression effect? Several methods have been recommended and discussed in the literature. First, since regression effect is expected to be the same for different treatments, comparisons between treatments should not be affected by the presence of such effect. To this end, the use of a control group in a study is highly desirable in the presence of patient selection (3). Unfortunately, there are situations when the inclusion of a control group in a study is not very practical, such as during the early Phase II development of a compound. Thus, alternative methods are needed.

Davis (7) showed that regression effect can be reduced by basing the selection of individuals on the average of a number of measurements rather than a single measurement. He gave an example using the cholesterol measurements. He demonstrated that the greatest reduction in regression effect occurred during the first four pretreatment measurements. After the fourth measurement, little gain can be achieved by taking additional pretreatment measurements. Another option suggested by Davis (7), Ederer (5), and Gardner and Heady (8) is to select patients on the basis of a first measurement but use a second measurement before treatment as the basis from which to compute the change. If the correlation coefficient between the posttreatment measurement and the first baseline is the same as that between the first and the second baseline, then there will be no expected mean change due to regression. Unfortunately, the mean percentage change using the second pretreatment measurement as the baseline can be infinite because the baseline is not truncated below.

Since the use of the average of several measurements as the baseline is clearly desirable from the point of reducing regression effect as well as the probability of misclassification, a half-way method between the previous two options was suggested by Davis (7) when there are a sufficient number, say k, of pretreatment

measurements. In the latter case, the average of the first k_1 measurements can be used to classify and select patients while the average of the rest k_2 ($k_1 + k_2 = k$) can be used to calculate the change. This will result in a smaller probability of misclassification and reduce regression effect at the same time.

If the observed results must be adjusted for regression effect, how should this be handled? Curnow (9) proposed a method to adjust the observed mean percentage change for regression effect. Curnow assumed the simplest case that the treatment effect, denoted by Δ ($E(Y) = E(X) - \Delta$, $\Delta > 0$), is the same regardless of the pretreatment values. In other words, the conditional mean percentage change under Curnow's assumption is

$$E\left(\frac{Y - X}{X} \mid X = x\right) = (\rho - 1)\left(1 - \frac{\mu}{x}\right) - \frac{\Delta}{x} \quad (4.1)$$

From (4.1), the mean percentage change for the top ($100f$) percentage of the X-distribution is found to be

$$E\left(\frac{Y - X}{X}\right) = E(R) = (\rho - 1) - \left(\frac{\rho - 1 + \frac{\Delta}{\mu}}{f}\right) \times \int_\tau^\infty \left(\frac{1}{1 + \frac{\sigma}{\mu} z}\right) \phi(z)dz \quad (4.2)$$

Let

$$K = \frac{1}{f}\int_\tau^\infty \left(\frac{1}{1 + \frac{\sigma}{\mu} z}\right) \phi(z)dz$$

The terms in (4.2) can be rearranged to arrive at the relationship in (4.3). If estimates for μ, σ, and ρ are available, an estimate can be obtained for the treatment-related mean percentage change by substituting the observed mean percentage change for $E(R)$ in (4.3). To obtain the observed mean percentage change, first compute the percentage change for each individual in the study and then average such computed percentage changes to obtain the observed mean percentage change $\hat{R}$.

$$\frac{\Delta}{\mu} = -\frac{E(R)}{K} + \frac{(1 - K)(\rho - 1)}{K} \quad (4.3)$$

Curnow (9) used the result in (4.3) to adjust an observed mean percentage reduction in fasting glucose. In the example he considered, a 15% mean reduction (ie, $\hat{R} = -15\%$) was observed in a study that enrolled patients with a single pretreatment glucose measurement that was among the top 1% of the glucose-distribution. The treatment-related mean percentage reduction was estimated to be 7.6% (down from 15%). Curnow found that any observed mean percentage decrease less than 9% (ie, $|\hat{R}| < 9\%$) in the study would give a negative estimate for the treatment effect, that is, the treatment did not reduce the glucose level.

Similarly, the observed mean change can be adjusted for regression effect in order to estimate the treatment-related mean change. Again, assuming a uniform treatment effect, the relationship in (4.4) can easily be derived from (3.2) for a study that selects the top $(100f)^{\text{th}}$ of the X-distribution based on one single pretreatment measurement. Accordingly, an estimate for the treatment-related mean change can be obtained by subtracting from the observed mean change a quantity that estimates the mean change from regression.

$$\Delta = -E(Y - X) + \frac{(\rho - 1)\sigma}{f} \frac{e^{-\frac{\tau^2}{2}}}{\sqrt{2\pi}} \quad (4.4)$$

A bivariate normal distribution is assumed in this paper to quantify and to adjust for regression effect. The bivariate normal distribution is only an approxima-

tion considering that the distributions of many measurements, especially those of biochemistries, are skewed either to the left or to the right. In general, the conditional distribution of Y given $X = x$ and the parameter values in the conditional distribution must be known in order to conduct the adjustment. The latter is not always an easy task.

COMMENTS AND QUESTIONS

When selection occurs, the observed mean (and mean percentage) difference is no longer an unbiased estimate for the relevant treatment effect. Methods of constructing treatment-related mean (and mean percentage) change were reviewed. Instead of adjusting the summary statistics, the individual observations can be adjusted as follows

Change: $Y - X - (\rho - 1)(x - \mu)$

Percentage change:

$$\frac{Y - X}{X} - (\rho - 1)\left(1 - \frac{\mu}{x}\right) \qquad (5.1)$$

After the individual observations are adjusted, the usual statistical summarization using the adjusted observations can be carried out, without concern about regression effect.

Thus far, this paper has assumed that high values are undesirable and patients with high values are therefore targeted for treatment intervention. The discussion on regression effect is equally applicable if low values are undesirable and patients with low values are selected for treatment instead. In the latter case, regression effect tends to pull the measurement up even in the absence of any treatment effect. As a result, the observed mean (and mean percentage) difference needs to be adjusted downwards in a manner similar to that described earlier in order to obtain an estimate for the treatment effect.

Efficacy evaluation is not the only place where regression effect can distort the picture. Regression effect impacts the interpretation of results from a safety analysis as well. Unlike the efficacy parameters, most protocols in the Phase II development of an investigational drug require subjects in a study to have *normal* labs or *clinically normal* labs prior to receiving any protocol medications. In terms of chemistry labs, this generally means that a subject cannot have too high a lab value in order to be eligible. Using the earlier notations, assume for the time being that a study excludes the top $(100f)\%$ of the X-distribution for a chemistry assay. With the expression for the conditional mean change in (2.2), it can be shown that the mean change under the bivariate normal assumption for the chemistry assay under consideration is

$$E(Y - X) = -\frac{(\rho - 1)\sigma}{f}\frac{e^{-\frac{T^2}{2}}}{\sqrt{2\pi}} \qquad (5.2)$$

where T again is the upper $(100f)^{\text{th}}$ percentile of the standard normal distribution. Notice that the mean change in (5.2) is the negative of that in (3.2). Thus, what affects the efficacy evaluation will also affect the safety analysis, but in the opposite way. In other words, there is a general tendency for the safety lab values to go up even though the intervention has absolutely no effect on the assay parameter. To adjust for regression effect, one needs to subtract from the observed change the quantity given in (5.2). Alternatively, one can conduct the adjustment in (5.1). The adjustment for regression effect in the analysis of safety labs is rarely done. As a result, the alarm may have been raised regarding the effect of a treatment on the safety labs more often than necessary. Similar statements apply to the use of percentage change.

Regression models other than that in (2.2) have also been proposed and considered by researchers. For example, James (3) proposed to relate $E(Y|X = x)$ to x by

$$E(Y \mid X = x) = \mu + \gamma\rho(x - \mu) \qquad (5.3)$$

where ρ again is the correlation coefficient between X and Y. The above relationship between $E(Y|X = x)$ and x was further explored by Senn and Brown (10) who proposed to use the maximum likelihood method to estimate the parameters in (5.3) instead of the method of moments originally proposed by James.

Clinical trials routinely select patients based on the primary efficacy variables. Even though the comparison between the treatment groups in a controlled randomized trial is not affected by regression effect, the discussion in this paper is nevertheless relevant whenever one attempts to estimate individual treatment effects. When reporting the mean change in the primary efficacy variable for a treatment group in the presence of patient selection, the reported mean change is subject to regression fallacy. This issue for 2×2 cross-over designs was discussed in a recent article by Grender, Johnson, and Elston (11). How the observed results, especially those coming from Phase II trials, can be systemically adjusted for regression effect represents a challenge to pharmaceutical statisticians.

It is not unusual for a clinical statistician to conduct a subgroup analysis. Even though a subgroup might be identified by factors that are not the primary efficacy variable, patient selection often takes place in the process of identifying the subgroups. In view of the discussion in this paper, the appropriateness of the inferences drawn from subgroup analyses needs to be examined carefully in light of the selection criterion and the regression toward the mean phenomenon.

It is generally agreed that, if at all possible, patient selection should be based on the average of several pretreatment measurements. Considering that the measurement of a clinical parameter is subject to both the biological variation over time and a random measurement error, the question of when multiple pretreatment measurements should be taken to capture these two sources of variation needs to be addressed. In addition, how does the cost of taking multiple pretreatment measurements compare to the benefit obtained from taking them? In the case of limited resources, should adjustment procedures be relied upon exclusively?

All of the above questions remain outstanding and deserve attention. The answers to them will undoubtedly impact on the design as well as the analysis of clinical trials.

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EXHIBIT Q

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Correcting for Regression to the Mean in Behavior and Ecology

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ABSTRACT: If two successive trait measurements have a less-than-perfect correlation, individuals or populations will, on average, tend to be closer to the mean on the second measurement (the so-called regression effect). Thus, there is a negative correlation between an individual's state at time 1 and the change in state from time 1 to time 2. In addition, whenever groups differ in their initial mean values, the expected change in the mean value from time 1 to time 2 will differ among the groups. For example, birds feeding nestlings lose weight, but initially heavier birds lose more weight than lighter birds, a result expected from the regression effect. In sexual selection, males who remain unmated in the first year are, on average, less attractive than mated males. The regression effect predicts that these males will increase their attractiveness in the second year more than mated males. In well-designed experiments, changes in the experimental and control groups would be compared. In observational studies, however, no such comparison is available, and expected differential effects must be accounted for before they can be attributed to external causes. We describe methods to correct for the regression effect and assess alternative causal explanations.

Keywords: density dependence, mating success, regression, statistical artifact.

Regression toward the mean occurs in repeated-measures analyses whenever the correlation between the measurements at different times is less than perfect. At the second measurement, individuals with values above the mean will, on average, have lower values, whereas those with values below the mean will, on average, have higher ones. This so-called regression effect was first discovered by Galton (1886) through observations of human stature and in sweet pea experiments. Galton noted that if there were no regression to the mean, random error would cause the population variation to increase over time and extraordinarily small and large individuals would accumulate in the population.

Regression to the mean complicates analyses for researchers who study subsets of the population selected on the basis of their initial measurement. For example, studies that target individuals with initially large values should expect these values to decrease based on regression to the mean. The problem arises when a researcher attributes this decrease to an intervention or other causal effect. Since regression to the mean will affect both experimental and control groups, a well-designed experimental study will not be subject to this problem. However, statistical corrections must be applied to observational studies that compare groups that differ in their initial mean values. In this article we describe such tests.

The regression effect predicts that very sick patients should feel better at the next measurement even without effective treatment and thus could explain some of the placebo effect (McDonald et al. 1983). Likewise, regression to the mean predicts that recall of rare events may be improved later and could explain some of imagination inflation, an increased confidence that an imagined childhood event has occurred (Pezdek and Eddy 2001). Tversky and Kahneman (1974) noted the regression effect in training programs: students who do very well on the first trial typically do worse on the second, and students who do poorly at first typically do better later. This has led some to the erroneous conclusion that rewarding success does not work but that punishing failure does, and it leads to "a lifelong schedule in which we are [apparently] most often rewarded for punishing others and punished for rewarding" (Kahneman and Tversky 1973, p. 251). Good (1990) extended this reasoning to international relations, suggesting that the regression fallacy could be responsible for war.

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Despite the widespread appreciation of the regression effect, recognizing its role in applications is difficult, even for statisticians well aware of the problem (Kahneman and Tversky 1973; Good 1990; Stigler 1997). For example, the statistician Secrist noticed that businesses with exceptional profits in one year tended to have smaller profits in the next and that businesses with very low profits tended to do better the next year. He used these data to conclude that all companies were converging toward mediocrity in his book *The Triumph of Mediocrity in Business* (Secrist 1933). Although the error was pointed out by Hotelling (1933) and later by Friedman (1992), other prominent economists have repeated it (Sharpe 1985; Fama and French 2000). Since regression to the mean is not likely to be recognized by sports gamblers, profitable betting opportunities may exist (Lee and Smith 2002).

A number of articles have pointed out problems due to regression effect in several disciplines, including epidemiology and clinical studies (Davis 1976; Curnow 1987; Bland and Altman 1994; Yudkin and Stratton 1996; Barnett et al. 2005), exercise and sports science (Shephard 2003; Nevill et al. 2004), psychiatry (Streiner 2001), chronobiology (Atkinson et al. 2001), communication (Zhang and Tomblin 2003), and hierarchical linear modeling (Marsh and Hau 2002). However, there has been no general survey in behavior and ecology. Here, we discuss several recent biological and ecological examples where causal explanations have been proposed for the regression effect. We describe a method that can be used to correct for the regression effect and illustrate its use in the examples.

Regression to the mean will be present whenever individuals or populations are measured at two different times. We note four general manifestations of regression to the mean that may be mistakenly attributed to causal factors. First, there is a negative correlation between an individual's first value and changes in that value between the first and second measurements (Cichoń et al. 1999). Second, there will be change in the mean of a single group whenever the mean of that group differs from the population mean. Third, there will be different changes in two groups whenever these two groups differ in their mean values at the first measurement. This applies even if both groups lie above the population mean or both groups lie below the population mean. Fourth, the most subtle artifact occurs when all individuals show an increase but those below the mean increase more than those above the mean (or all decrease, but those above the mean show a greater decrease than those below the mean; Gebhardt-Henrich et al. 1998; Griffith and Sheldon 2001).

We describe examples of both observational and experimental studies that illustrate these problems. Our examples include changes in mass, sexually selected traits, costs of cooperative breeding, and density dependence in forests. We have not been provided with data sets for all these examples and are not able to reanalyze all of them. We start by describing some of the examples.

Examples

Mass Loss

Our primary example considers the debate on the correct statistical method to test whether mass loss depends on initial mass (Cichoń et al. 1999; Gebhardt-Henrich 2000; Ruf 2000). Contrary to other examples, change is defined here as the initial state minus the final state (corresponding to mass loss), and regression to the mean implies a positive correlation between the initial weight and the mass lost. Indeed, a positive correlation between incubation mass and subsequent mass loss in birds has been noted in several observational studies (Norberg 1981; Nur 1988; Hillström 1995; Merilä and Wiggins 1997; Gebhardt-Henrich et al. 1998). An example is in figure 1 (*top*). This result has often been interpreted as "parents in initially better condition can 'afford' to lose more mass than those in poorer condition, and that this energy can be allocated to their offspring" (Cichoń et al. 1999, p. 191), but it is expected from the regression effect (Cichoń et al. 1999). Cichoń et al. (1999) developed a method to correct for the regression effect based on resampling. Their method assumed that the initial and final values were uncorrelated, which is equivalent to complete regression to the mean. As noted by Ruf (2000), this is a very unreasonable assumption. Gebhardt-Henrich (2000) suggested an alternative statistical analysis that also assumes independence of the initial and final measurements.

Sexual Selection

In a noncontrolled experiment, Witte and Curio (1999) measured the attractiveness of male Javanese mannikins (*Lonchura leucogastroides*) to females. They then attached red feathers to the males' crowns and found that previously unattractive males gained attractiveness, whereas previously attractive males lost attractiveness. There was a significant negative relationship ($r = -0.76$, $n = 11$, $P = .003$) between initial attractiveness and change in attractiveness. Witte and Curio suggested that this is because attractive males have their phenotype disrupted by addition of what is otherwise an inherently attractive trait. However, as a result of regression to the mean, such a relationship is expected when there is no effect of the experiment.

In another example from sexual selection, Blows (1998) examined the mating success of hybrid *Drosophila serrata* × *Drosophila birchii* when male or female hybrids

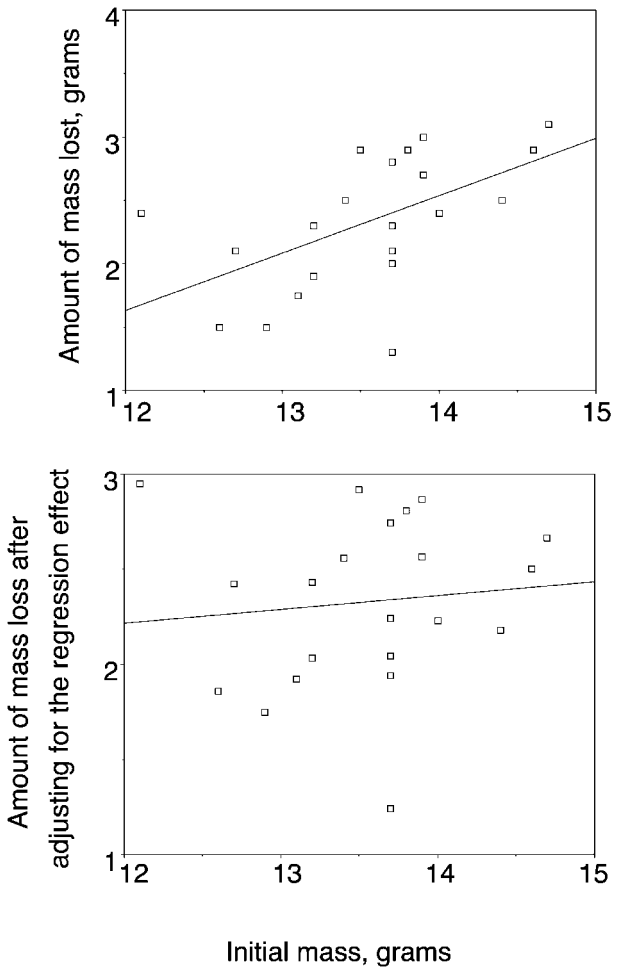

Figure 1: Loss of mass of female blue tits during the raising of their brood. *Top*, original data (from Gebhardt-Henrich et al. 1998). *Bottom*, mass loss adjusted for the regression effect using equation (6).

were placed with *D. serrata* in 29 replicated lines. Blows compared hybrid mating success five generations after initiating the hybrid lines with hybrid mating success 19 generations later. Among females, he found a tendency for hybrid lines with high mating success at generation 5 to have lower success at generation 24 and for those with low mating success at generation 5 to have higher success at generation 24. In males, there was a general increase in mating success between the two generations (fig. 2), but males with low mating success increased their mating success to a greater extent. Blows (1998) concluded that all lines were evolving toward a single regression line that describes a linear association between mean male mating success and mean female mating success. Blows considered the possibility that the regression effect may have influenced the results. He tested whether each line had significantly changed its mating success between generation 5 and generation 24 with a two-sample *t*-test. The results of these *t*-tests were combined into a single *P* value. The combined *P* value suggested that the mating success of the lines had indeed changed during the course of the experiment, but this does not in itself control for the regression effect, which also predicts a change.

Badyaev and Duckworth (2003) measured area of red on the breast of male house finches (*Carpodacus mexicanus*) to investigate how prior mating status affects an individual's investment in sexual ornaments. The red area increased from one year to the next, but it increased more for unpaired males than for paired males. Badyaev and Duckworth (2003) concluded that prior mating status had a strong influence on future development of sexually selected traits. However, red area is an attractive trait to females (Hill 1994), so paired males presumably initially had more red area than unpaired males. A greater increase among the unpaired group is expected from the regression effect, and this must be accounted for before differences can be assigned to mating status. Before considering these and other examples in more depth, we now consider methods to correct for the regression effect.

Theory

Most methods to adjust for the regression effect deal with the situation in which study subjects are selected on the basis of a large (or small) initial measurement (James 1973;

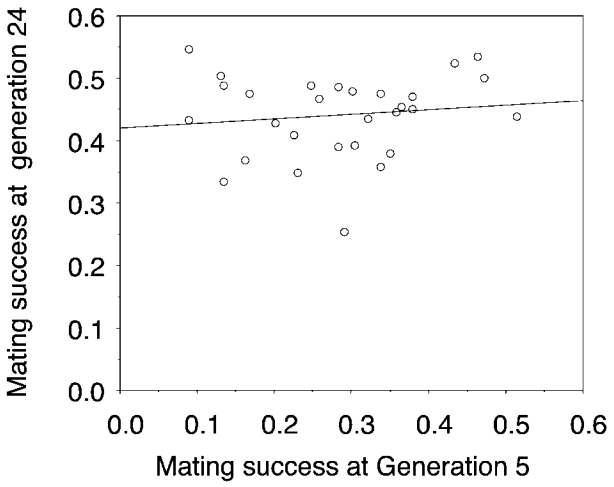

Figure 2: Hybrid male (*Drosophila serrata* × *Drosophila birchii*) mating success measured as the proportion of female *D. serrata* inseminated in 29 lines, with each line measured at generation 5 and again at generation 24 (Blows 1998). The data here have been arcsine–square root transformed. Note the much larger variance at generation 5 ($s_1^2 = 0.013$) than at generation 24 ($s_2^2 = 0.004$).

Senn and Brown 1985; Curnow 1987; Mee and Chua 1991; Chen and Cox 1992; Chuang-Stein 1993; Chen et al. 1998; Naranjo and McKean 2001). Here we consider studies in which there is no initial selection bias but where individuals with different initial values are separated and compared with respect to their subsequent values. The question of interest in these studies is, are there differential effects on the groups with initially low and high values beyond that expected from the regression effect? As pointed out by Galton (1886) and Hotelling (1933), a differential effect would change the variance of the population. Thus, if heavy individuals lose more weight than expected by regression to the mean and/or lighter individuals gain more, we expect convergence to the mean and a corresponding reduction in variance. If heavy individuals lose less weight than expected and/or lighter individuals gain less, we expect an increased variance in the population. Thus, a suitable test for a differential effect is a test of the equality of variance in the two time groups. We outline the test following Berry et al. (1984) and Chuang-Stein (1993).

Let the measurements at times 1 and 2 be X_1 and X_2 and the change in these measurements between time intervals be $D = X_1 - X_2$. We assume that the measurements have a bivariate normal distribution with means μ_1 and μ_2, variances σ_1^2 and σ_2^2, and correlation ρ. We define an additive effect as one that affects all subjects equally; that is, it is the mean difference between the two sets of measurements, $\Delta = \mu_1 - \mu_2$. When measuring body mass at two time points, this is the average mass loss. We define a differential effect as one that affects individuals differently based on their initial values, X_1. If there is a differential effect that pushes values toward the mean (above and beyond the regression effect), then the variance at time 2 will be smaller than that at time 1: $\sigma_1^2 > \sigma_2^2$. If there is no differential effect, then the variances will be the same: $\sigma_1^2 = \sigma_2^2$. When there is only an additive effect and no differential effect, the expected change from time 1 to time 2 of the individual values, D, is given by the regression function of D on X_1,

$$\mathrm{E}[D|X_1] = (1 - \rho)(X_1 - \mu_1) + \Delta, \tag{1}$$

and the expected percentage change is

$$\mathrm{E}\left[\frac{D}{X_1}|X_1\right] = (1 - \rho)\left(1 - \frac{\mu_1}{X_1}\right) + \frac{\Delta}{X_1}. \tag{2}$$

These equations show that the expected change (and the expected percentage change) is positive for values above the mean and negative for values below the mean and that the magnitude of the expected change increases for measurements X_1 farther from the mean. The correlation between the initial value and the change is

$$\sqrt{\frac{1 - \rho}{2}}. \tag{3}$$

The magnitude of the differential effect is measured as the ratio of the standard deviations of the two sets of observations, $\theta = \sigma_2/\sigma_1$. Assuming only that the measurements are bivariate normal, we obtain the following relationship between the initial state and the change in state:

$$\mathrm{E}[D|X_1] = (1 - \rho\theta)(X_1 - \mu_1) + \Delta. \tag{4}$$

Note that $\rho\theta$ is the regression of X_2 on X_1. If $\theta = 1$, there is no differential treatment effect: individuals above and below the mean decrease or increase in a manner that is expected from the regression effect.

Following Berry et al. (1984), we test for a differential effect by testing the null hypothesis H_0 ($\sigma_1^2 = \sigma_2^2$) against the alternative H_a ($\sigma_1^2 \neq \sigma_2^2$) with Pitman's (1939) test for the equality of variances in paired samples,

$$T = \frac{\sqrt{n - 2}[(s_1/s_2) - (s_2/s_1)]}{2\sqrt{1 - r^2}}, \tag{5}$$

where T has a Student's t distribution with $n - 2$ degrees of freedom, n is the sample size, and s_1, s_2, and r are the usual estimates of σ_1, σ_2, and ρ, respectively.

We can also adjust each value by subtracting the change that is expected as a result of the regression effect, $D^* = D - \mathrm{E}[X_1 - X_2|X_1]$. From equation (1), we estimate D^* with

$$\hat{D}^* = \hat{\rho}(X_1 - \bar{X}_1) - (X_2 - \bar{X}_2), \tag{6}$$

where $\hat{\rho} = r$ if the null hypothesis of equal variances is rejected and

$$\hat{\rho} = \frac{2rs_1s_2}{s_1^2 + s_2^2}$$

when it is not rejected. The adjusted differences, $\hat{D}^*$, can be regressed against X_1 and graphically viewed in a scatter plot, or they can be used to test for the influence of a measured factor that is correlated with the initial measurements. This is illustrated further in the next section.

Analysis of Examples

In this section, we reanalyze examples for which we have been able to obtain data from the original article or have been provided the data by the authors.

Mass Loss

Gebhardt-Henrich et al. (1998) presented data on loss of mass of female blue tits *Parus caeruleus* during the feeding of the brood. Their figure 2, reproduced here as figure 1 (*top*), shows that all blue tits lost mass but that females that were initially heavier lost more mass than those that were initially lighter (correlation between mass and loss of mass is $r = 0.55$, $P = .0096$). We analyzed these data using equation (5) and could find no evidence for a differential effect ($T = 0.74$, df $= 19$, $P = .46$). We constructed adjusted values by using equation (6) and added these values to the change in the mean (fig. 1, *bottom*). This shows that there is a lack of association between change in mass and initial mass after the regression effect has been accounted for. Note that these adjusted values could also be used in other tests. For example, some females may be in poor habitat and initially of less weight than others in good habitat. If one wished to determine if habitat influenced weight loss, a simple *t*-test comparing females in the two habitats would be confounded with the regression effect. One approach to deal with this would be to compare the adjusted values (fig. 1, *bottom*) between females in poor and good habitats using a *t*-test.

Cichoń et al. (1999) presented initial mass and mass loss from three other studies (on the blue tit, the collared flycatcher *Ficedula albicollis*, and the pied flycatcher *Ficedula hypoleuca*). They concluded that although the correlation between initial mass and mass loss is positive, for all three species the observed empirical correlations were significantly weaker than those expected under null expectations derived by bootstrapping (under the assumption of independent initial and final masses). The result led them to suggest that lighter birds actually lost more mass than expected and that heavy birds lost less mass than expected. Their correction thus led to a conclusion that was opposite to that reached by the original analysis, but this conclusion depends on an obviously falsifiable null model that initial and final weights are uncorrelated (Ruf 2000). We applied the tests outlined in this article and found evidence of differential mass loss only in the collared flycatcher ($T = 2.22$, df $= 273$, $P = .04$). In this species, the correlation between initial and final mass was $r = 0.33$. Heavier birds lost more mass than expected (variance of values at the second measurement was 77% that of values at the first measurement), opposite to the conclusions of Cichoń et al. (1999).

Sexual Selection

We found no evidence that male mating success is affected by addition of a red feather in Witte and Curio's (1999) study of Javanese mannikins ($T = 0.403$, df $= 9$, two-tailed $P = .696$). We emphasize that other results in the article stand, and the article should be consulted to evaluate this result in context.

In the study by Blows (1998), the correlation between (arcsine–square root transformed) hybrid female mating success in generation 5 and change in hybrid female mating success between generation 5 and generation 24 was $r = -0.78$ ($n = 29$ replicate lines). It is not possible to exclude the regression effect as the underlying reason for this correlation ($T = 1.081$, df $= 27$, two-tailed $P = 0.289$). Indeed, the low correlation between hybrid female mating success at generation 5 and generation 24 ($r = -0.06$) results in a large regression effect (see eq. [1]).

In males, there was a general increase in mating success between the two generations (fig. 2), but males with low mating success increased their mating success to a greater extent. The correlation between (arcsine transformed) mating success at generation 5 and change in mating success is $r = -0.85$. In this example, the male lines have converged. The variance decreased by 67% (from 0.013 to 0.004 on the transformed scale; see fig. 2), and this is significant ($T = 3.051$, df $= 27$, $P = .005$). This implies that males with particularly low values of mating success at generation 5 are indeed increasing their mating success more than those with high mating success. Thus, there is support for Blows's conclusion that all lines were converging toward a common trajectory, at least through male mating success.

Other Examples

Griffith and Sheldon (2001) found a negative correlation between the size of an unpigmented plumage patch in male collared flycatchers and change in the size of the patch across a season ($r = -0.4$, $n = 80$). Although they did not attach much significance to this finding, they have kindly provided us with the data, and we find that the result can be explained by the regression effect ($T = 0.51$, df $= 78$, $P = .61$).

Discussion

Regression to the mean results whenever there is a less-than-perfect correlation between successive measurements. In fact, the magnitude of the regression to the mean is proportional to $(1 - \rho)$. Because measurement error lowers the correlation, regression to the mean can be reduced by using more accurate measurement methods or

the mean of replicate measurements (Gardner and Heady 1973; Blomqvist 1987; Griffith and Sheldon 2001). However, even when this is done, the regression effect will be present because the correlation will be less than perfect, and the best approach to avoiding the regression fallacy is to use an appropriate control and compare changes in the experimental and control groups. Thus, in the study by Witte and Curio (1999), it would have been possible to examine changes in a group of nonmanipulated males and see if they differed from those in the manipulated males. Russell et al. (2003) found that in cooperative meerkats *Suricata suricatta,* individuals investing heavily in one breeding event significantly reduce their contribution in the following event, whereas those previously investing little significantly increase their contribution. They recognized that this could be due to the regression effect and used a supplemental feeding experiment (as well as other correlative evidence) to infer causality.

In observational studies, a measured causal factor can be investigated by examining its relationship with values adjusted for the regression effect. This would be possible in the house finch study by Badyaev and Duckworth (2003), for example. In this study, unpaired males increased the area of red in their plumage 85% more than the paired males did. Assuming that they initially had less red in their plumage, a differential increase is expected. The causal factor (mating status) is known, so the adjusted values could be calculated according to equation (6) and then compared using a *t*-test. Unfortunately, we have not been provided with the original data for this study, and the information needed to correct for the regression effect is not extractable from the article. In other studies (e.g., those of mass loss, where larger individuals are postulated to be in better "condition"), the causal factor is not measured, and only the change-in-variance test can be used.

Wills et al. (1997) and Wills and Condit (1999) suggested that strong density-dependent effects were occurring in tropical trees. They found that quadrats in experimental plots with few individuals of one species tended to recruit more individuals than quadrats with many individuals. They interpreted this as a result of density dependence, perhaps acting through parasites and pathogens; again the causal factor is postulated but not measured. There have been many other studies purportedly demonstrating a role of density dependence in enabling coexistence of tree species (Lambers et al. 2002). Indeed, Lambers et al. (2002) suggested several reasons why density dependence may be underestimated. In general, the supporting evidence relies on the finding of a negative correlation between recruitment and prior density of trees in a quadrat, which would be predicted based on the regression effect. The possibility of regression effects can be quite subtle. Lambers et al. (2002) measured the proportion of seeds germinated in 1 × 1-m plots and showed that this was negatively correlated with density of seeds in the plot. However, if high density of seed is partly a result of high germination at a previous time period, then a negative correlation is expected from the regression effect.

We have not been able to analyze results on trees. However, in this case it is worth noting that at equilibrium, the variance in tree distributions across quadrats remains the same from one generation to the next (at least when measured at the same life-history stage). This means that it is impossible to refute the regression effect as a cause of a negative association between change and initial value and hence impossible to detect density dependence by these methods. Variance decreases due to high-density quadrats decreasing in number and low-density quadrats increasing in number must be compensated for by intermediate quadrats both increasing and decreasing. Unless one is inclined to invoke special factors causing all these changes, it is more parsimonious to assign all increases and decreases to random factors. More sophisticated methods using time series data that explicitly incorporate an error term to account for regression toward the mean are required if density dependence is to be detected (Lande et al. 2002).

Whenever two sets of measurements are not perfectly correlated, there will be regression toward the mean. Thus, ascribing biological significance to regression to the mean is equivalent to ascribing significance to a correlation of less than unity. Many traits are influenced by multiple factors, so correlations are rarely unity, and regression toward the mean is inevitable.

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UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF KENTUCKY
LOUISVILLE DIVISION

Chelsey Nelson Photography LLC and **Chelsey Nelson,** Plaintiffs, **v.** **Louisville/Jefferson County Metro Government; Louisville Metro Human Relations Commission-Enforcement; Louisville Metro Human Relations Commission-Advocacy; Verná Goatley,** in her official capacity as Executive Director of the Louisville Metro Human Relations Commission-Enforcement; and **Marie Dever, Kevin Delahanty, Charles Lanier, Sr., Leslie Faust, William Sutter, Ibrahim Syed,** and **Leonard Thomas,** in their official capacities as members of the Louisville Metro Human Relations Commission-Enforcement, Defendants.	**Case No. 3:19-cv-00851-BJB-CHL** **[Proposed] Order Granting Plaintiffs' Motion to Exclude Testimony of Netta Barak-Corren**

This matter is before the Court on Plaintiffs' Motion to Exclude Testimony of Netta Barak-Corren. The Court, having reviewed the motion and being otherwise sufficiently advised, orders as follows:

IT IS HEREBY ORDERED

1. Plaintiffs' Motion to Exclude Testimony of Netta Barak-Corren is GRANTED. Accordingly, Professor Netta Barak-Corren's testimony, including her report dated June 30, 2021, her article *A License to Discriminate: The Market Response to Masterpiece Cakeshop* forthcoming in the Harvard Civil Rights-Civil

Liberties Law Review, her article *Religious Exemptions Increase Discrimination Towards Same–Sex Couples: Evidence from Masterpiece Cakeshop* forthcoming in the Journal of Legal Studies, her Online Appendix, and any other related testimony shall be excluded in this matter.